THE JOURNAL OF PARAPSYCHOLOGY

Volume 16 MARCH, 1952 Number 1

Editorial PAGE

The Challenge of the Dowsing Rod 1

Multiple Aspect Targets in Tests of ESP 11
ESTHER BOND FOSTER

Thought Transference and Related Phenomena 23
R. H. THOULESS

Minor Articles

On the Susceptibility of a Manually Operated Random Selector to Psi Dexterity: A Preliminary Report 41
E. A. G. KNOWLES

A Further Study of the PK Placement Effect 59
H. FORWALD

Book Reviews

Det Ockulta Problemet by John Björkhem 68
W. G. ROLL

Vorderingen En Problemen Van Parapsychologie by F. A. Heyn and J. J. Mulckhuyse 71
HENDRIK RUBINGH

Glossary 73

A SCIENTIFIC QUARTERLY DEALING WITH EXTRASENSORY PERCEPTION, THE PSYCHOKINETIC EFFECT, AND RELATED TOPICS

DUKE UNIVERSITY PRESS
DURHAM, N. C.

The Journal *of* Parapsychology

EDITORIAL STAFF

J. B. RHINE, J. G. PRATT, and BETTY M. HUMPHREY, *Editors*
J. A. GREENWOOD and T. N. E. GREVILLE, *Statistical Editors*
DOROTHY H. POPE, *Managing Editor*

ADVISORY EDITORIAL BOARD

HORNELL HART, Duke University
H. H. PRICE, University of Oxford
GARDNER MURPHY, College of the City of New York
ROBERT H. THOULESS, University of Cambridge

This JOURNAL is published on the fifteenth day of March, June, September, and December by Duke University Press, Durham, North Carolina.

Contributions submitted for publication and all editorial communications should be addressed to the Managing Editor, Dorothy H. Pope, College Station, Durham, North Carolina. Correspondence with the editors is advised before submitting articles other than reports of experimentation. Since it does not forward manuscripts by registered mail, the JOURNAL cannot guarantee that they will not be lost in transit, and contributors are urged to keep copies of their papers. All contributions should be typewritten, double spaced References should be given in the form adopted by the JOURNAL.

Reprints may be ordered when the proof is returned. The JOURNAL will bear one half the cost of reprints up to two hundred copies. Correspondence concerning subscriptions, change of address, back numbers, and other business communications should be addressed to Dorothy H. Pope, College Station, Durham, N. C.

The subscription price is $6.00 a year; single current numbers $1.50. The rate for back volumes is $7.00; for single numbers $1.75. Missing numbers will be supplied free when lost in the mails if written notice is given within one month of the date of issue. All remittances should be made payable to the JOURNAL OF PARAPSYCHOLOGY.

Subscriptions may be obtained in Great Britain through the Cambridge University Press, Bentley House, Euston Road, London, N. W. 1.

Entered as second class matter May 21, 1942, at the post office at Durham, N. C., under the act of March 3, 1879.

DUKE UNIVERSITY PRESS

The Journal *of*
Parapsychology

Volume 16 MARCH, 1952 Number 1

EDITORIAL

THE CHALLENGE OF THE DOWSING ROD

THERE has been more public interest during the past two years in the subject of dowsing than ever before. It has been due mainly to the publication of Kenneth Roberts' book *Henry Gross and His Dowsing Rod* and the many newspaper and magazine articles and reviews that have followed. Some of this reaction is doubtless attributable to the fact that the book was written by a well-known writer, but in great part it is due to the spectacular character of the material it contains. The outstanding feature it presented was the report that fresh-water wells were successfully located by Henry Gross on the comparatively dry island of Bermuda, and that the dowsing was first done on a map of the island.

Another part of this public reaction is due to surprise and concern that science has neglected so old and widespread a practice. Some sympathy has thus been generated for the cause with which Roberts has identified himself. According to the more conventional sciences, however, dowsing would of course be considered not only to be unscientific but even to be rooted in magic and superstition. Generally speaking, therefore, it is no more to be expected that the profession of geology should have investigated dowsing than that psychologists should have taken a positive interest in claims regarding psi phenomena in general.

What does call for explanation, however, is the fact that parapsychologists have not undertaken to clear up the problems raised by the practice of the dowser. There has been some attention given to it by a few explorers in that field, it is true, notably by Sir Wil-

liam Barrett and Professor Charles Richet. The book entitled *The Divining Rod,* which Barrett wrote in conjunction with Theodore Besterman, is still the nearest approach to a standard work on the subject. Among other inquiries Barrett himself took a few dowsers into unfamiliar territory for tests in locating underground water. He obtained independent determinations over the same area by different dowsers, and even drilled a few control (dry) holes. However, even Barrett cannot be credited with experiments adequate to establish the case for dowsing.

There have in fact been only what must be classed as minor exploratory experiments on the dowsing problem thus far. Dr. G. A. M. Lintott of Guys Hospital in London published a report (*Guys Hospital Gazette,* June 24, 1933) of some experiments conducted by himself and some of his colleagues that involved dowsing over the water pipes in the hospital to test whether the water was flowing or standing still. He concluded that reliable indications were given by the movement of the dowsing rod.

The *Parapsychology Bulletin* of November, 1950, reported the significant results of an experiment with dowsing carried out by Miss Kathleen Kirby of the Harrogate Training College in England. Miss Kirby had her subject use the dowsing rod to locate pennies concealed under one of a number of pieces of cardboard. A statistical method was used in the evaluation of the results.

In the JOURNAL OF PARAPSYCHOLOGY of December, 1950, J. B. Rhine gave a report of a series of dowsing tests embodying a total of 170 trials, a series comparatively large as dowsing tests go. Most of these tests were conducted with Henry Gross, the dowser about whom Kenneth Roberts wrote his book, and the dowsing rod was used to determine whether or not the water was flowing in the underground pipes. The results thus obtained (with Gross and one other subject) were peculiar in that the number of successes was significantly and consistently below the average to be expected from pure chance.

Not all these short excursions into the dowsing field, however, have yielded results that were beyond the range of chance as an explanation. Dr. Gardner Murphy and his associates reported in the January, 1951, issue of the *Journal of the American Society for*

Psychical Research an investigation in which a group of dowsers estimated the depth at which water would be found in a specified area. The records of the tests showed only chance results.

Many inquiries too have been casual and, if reported at all, have been given only incidental mention. Dr. Jerome Alexander, editor of *Colloid Chemistry: Theoretical and Applied*, Vol. V (New York, 1944), states (p. 3) that on discovering that the dowsing rod dipped for him at the same places it did for a dowser whose performance he had witnessed, he "had about ten persons, unacquainted with the dowser's findings, make the same experiment. Most of them got the same response as the dowser, with the same inward twist of the wrist." This, in an informal way, represents another method of testing, that of measuring consistency of response, regardless of whether or not there is any water at the points indicated.

Sir J. J. Thomson, eminent physicist and Master of Trinity College, Cambridge, in discussing water dowsing in his *Recollections and Reflections* (1937), expressed the opinion that experiments on the problem are well worth making. He told the story of the employment of a dowser by Trinity College of Cambridge University (the college of Sir Isaac Newton). The account states that a geologist was engaged to indicate where a well ought to be sunk on one of the farms belonging to the College. The Senior Bursar, who was also a Fellow of the Royal Society, had charge of the operation; and after failure on the part of the geologist to find water, a local dowser was employed. He was able to direct them to water without difficulty. Although this method of testing the ability of the dowser was not well controlled, the experience of success following the failure of the geologist gives some ground for suspecting the presence of an unknown principle of importance and value. Moreover, there have been many attempts to harmonize dowsing with current physical theory by means of rational speculation, sometimes of a very liberal variety. (Among these are the writings of Dr. W. W. Tromp, e.g., *Psychical Physics*; and of J. C. Maby and T. B. Franklin, *The Physics of the Divining Rod*.)

It is indeed a good question to ask why so challenging a problem escaped the interest of such a concentration of scientists as Trinity College represented and, for that matter, the whole of mod-

ern science up to now. Altogether there has been at least enough evidence, such as it is, to justify further inquiry, and there has been no lack of dowsers willing to demonstrate the performance. Accordingly, the reasons for the scientific neglect of the problem must be sought elsewhere. They are not the usual ones. In anything which promises such immediate practical application and profitable consequences as dowsing does, if successful, we expect the barriers of ordinary prejudice to give way eventually if the claim has a valid basis.

It is hardly likely therefore that the scientific investigation of dowsing has been held back by any serious bias such as that encountered by other parapsychological claims, particularly those that clearly challenge physical explanation. Rather, it is now fairly apparent that *the neglect of the problem of dowsing by conventional science has been due to the difficulty of coming to grips with it in a successful and convincing manner under conditions that are clear-cut enough to warrant conclusions.*

This difficulty may be seen by examining the types of approach that have been used in investigations of dowsing. In her "Review of the Evidence for Dowsing" in the September, 1947, JOURNAL OF PARAPSYCHOLOGY Elizabeth McMahan classified these types under seven headings: "(1) Successful wells are located by dowsers near dry wells that are deeper. (2) The dowser succeeds where a geologist has failed. (3) The dowser accurately determines the depth of the water below the surface, especially in cases in which geological advice is contrary. (4) The same spot is located by two or more independent dowsers or by a dowser blindfolded. (5) Dowsing experiments are carried out in which a well is sunk at one spot where the dowser has indicated water and another sunk at a different spot where he has indicated no water. (6) The dowser accurately determines whether or not water is flowing in pipes. (7) Lost or hidden objects are located."

Four of these seven methods involve the sinking of wells and thus far no one has been prepared to run a sufficiently controlled and extensive investigation using the well-drilling method to give this necessarily costly approach a conclusive trial. An adequate research on these lines is not at all easy to design without involving

what could be for a pioneer research a prohibitive order of expense. The other methods, those involving the location of hidden objects (like the Kirby experiment), those using two or more independent dowsers to check the same spot (as in the Alexander test), and those using the test over underground pipes (as in Lintott's and Rhine's tests), have not been numerous and extensive enough to make a compelling and conclusive case.

But why have these easier and more easily interpreted methods not been more vigorously utilized? In answer it must be pointed out that all those experiments in which the reports show that controls were used and statistical analyses applied to the results, the percentage of success was far from that which the dowser claimed to have achieved under his ordinary working conditions. No dowser would be able to succeed in a practical way at this art if he had no better percentage of good results than those obtained by Kirby and by Rhine (Lintott's figures were not published), statistically significant though these are. The dowser who believes himself infallible in locating underground water in the field consequently does not ordinarily want to continue through a long series of controlled tests when the first few trials reveal a high percentage of errors. He can seldom appreciate the scientific experimenter's point of view, and any adequately designed testing program becomes difficult from there on. Many ambitious efforts have died a quick death when confronted with this situation.

No safe appraisal of the value of dowsing can be made from popular practice and opinion alone, that is, without experimental investigation. There are too many possibilities of error. Naturally we hear more about the remarkable successes of the dowser, especially if he was called in after costly failures to locate water by other methods. Unprejudiced and impartial judgments on matters of this kind are difficult to obtain, either from the dowser or the state geologist even with the best of intent. For one thing, no one knows how much allowance to make for the advantage given with the second choice the dowser has when he is called in after others have failed.

For what they may be worth there are some comparative figures that were published in 1939 by the New South Wales Water Con-

servation and Irrigation Commission. In their report quoted in *Water Works Engineering* for May, 1944, they give a record of twenty-two years of well-drilling in which the wells were about equally divided between those located by the divining rod and those that were not, a total of approximately thirty-five hundred wells for the period. The absolute failures were 14.7 per cent for the divined wells, and 7.5 per cent for the nondivined. Of the divined wells 70.4 per cent yielded over 100 gallons an hour, and of the nondivined wells 83.9 per cent. The figures were definitely better for nondivined wells, but it is important to know more about them. We should also know who were competing with the dowsers and how many of them were geologists. Otherwise we do not know whether these percentages were good or bad for dowsing. The dowsing enthusiast might say that these data do not speak well for the quality of the dowsers of the New South Wales area, but that would depend a great deal on the difficulty of locating water in the area concerned. We can only say that we do not yet have any adequate basis for judging the case for the divining rod.

* * * * *

From the foregoing summary it can be seen that we have not progressed much if any beyond the following position taken by Barrett and Besterman at the conclusion of their book, *The Divining Rod*, in 1926: "The dowser, in our opinion, is a person endowed with a subconscious supernormal cognitive faculty, which, its nature being unknown, we call, after Professor Richet, cryptesthesia.[1] By means of this cryptesthesia knowledge of whatever object is searched for enters the dowser's subconsciousness and is revealed by means of an unconscious muscular reaction, or less often by an obscure nervous sensation or emotion which produces physiological disturbances, or very rarely by means of direct supernormal cognition made conscious by a visualisation or hallucination.

"We do not believe that the accumulation of further masses of evidence, though of course this is not undesirable, will make the argument for these contentions any stronger. All that is required is the discovery of some fruitful generalisation which will permit the

[1] A term covered by our use of "extrasensory perception."

orthodox scientist to incorporate cryptesthesia into the canon of accepted and indisputable scientific knowledge."[2]

Elsewhere they go further into their view of the explanation:

"In short, we claim that dowsing is a purely psychological problem, that all its phenomena find their origin in the dowser's mind, that no physical theory can bear close consideration, and that the movements of the rod and of the dowser have no more direct relation to the discovery of, say, water than as giving physical and visible expression to a mental and abstract cognition."[3]

These views were essentially similar to those of Professor Richet and other distinguished scientists who had looked into the subject before Barrett and Besterman wrote their book. Before and since its publication there has, to be sure, been a great confusion of speculative hypotheses, especially as to the way in which the rod is propelled. It would, however, be gross folly indeed for the scientific student of today to allow his attention to be distracted to this wholly superficial aspect of the dowsing performance. A survey of the range of types of physical procedure used in different countries and in different parts of the same country readily reveals how misleading it would be to attach any significance to the type of movement, which is a mere form of expression. In one country a pendulum is used instead of a twig. One group of dowsers will use a forked twig, another a straight one bent into a U-shape. In one locality the practice will be to have the rod turn downward for water, in another it will turn upward. There are dowsers of local reputation who insist on having a green twig, while to others it will not matter whether it is green or not. To one dowser or one community of dowsers the twig must come from a certain species of tree; to another group that species will not do at all. Many dowsers use metal and not always the same metal. Dowsers of repute have been known to use nothing but the hands and there are those who claim to "see" the underground water or other substance or to experience a distinctive sensation when passing over the "vein" of water.

Students of psychology who are acquainted with the wide range of automatisms possible to those large numbers of people who dissociate easily will have no difficulty in accepting the possibility that

[2] P. 275.
[3] P. 267.

the dowser may be quite unconscious of his own muscular movement in causing the rod to turn. If the rod is held a certain way, a slight inward turn of the wrists will create an exaggerated movement, and it is not difficult for the dowser to believe that the rod is moving of itself or being pulled. The illusion can be easily created then that the rod moves in spite of the holder's efforts to restrain it. All that is needed is unconsciousness of the muscular action. There are methods of registering the true character of the action, however, and of showing plainly enough that it is automatic and that the dowser himself is merely unconscious of it.

What concerns parapsychology—as distinguished from conventional psychology, which can already account for the automatic muscular movement—is the claim that the rod or pendulum locates something not available to the senses or to rational inference. With the independent establishment of the occurrence of ESP there is nothing new in this hypothesis for the field of parapsychology. Our principal concern as parapsychologists is to inquire whether there is any advantage in this particular mode of registering ESP. While the published records do not help much to throw light on the comparative value of ESP response through automatisms and through normal conscious means, there have been to our knowledge a number of serious efforts through the years to put the matter to test, especially through the use of automatic writing. In all of these cases we may be sure that if any superior order of success had been shown, a great deal would have been made of the fact. These results appear, rather, to have been of the order of those mentioned in the statistically estimated series (of Kirby and of Rhine) and not to have had the high percentages of success claimed by the dowser for his field practice.

We can, then, at this point, either take as a hypothesis the view that the remarkable, almost perfect, results credited to certain of the more famous dowsers are exaggerated or that there is something in the work of the dowser in his own local practice that permits ESP to operate at its best—something interfered with by the attempt to control it and subject it to laboratory safeguards.

The latter of these two attitudes seems the preferable one to take, if only because nothing is to be lost by trying it out; and there is

something at least to support it. In general, experimental controls do interfere with the natural phenomenon to be studied and this may be extremely important with the delicate capacities involved in dowsing. We know that ESP is very elusive in the forms of expression with which we are already familiar in parapsychology. We may well expect the dowser to be as easily interfered with as any other parapsychological subject, and we must assume that he would not himself be any more aware of conditions that are adverse to his art than he is of the fact that he is himself actually exerting the muscular action which causes the rod to turn. The ability, if he has it, operates quite unconsciously, both in its ESP and in its muscular aspect.

It is important to keep this fact in mind for it will keep the experimenter from relying unduly upon what the dowser himself says as to what he can do, the conditions under which he can work, and the like. If we recognize that the dowser may have no more idea himself than anyone else as to what is going on and what the limits of his capacities are, we shall be on safer ground in making further studies. This is not to ignore the necessity of respecting the dowser's own philosophy of his ability in planning the best type of test. He should not be asked to go against his beliefs.

If, now, current interest in the art of dowsing is sufficient to sustain an adequate investigation, the information that is already available on the subject should be made use of to the limit, after due sifting and reappraisal. Especially should it be reconsidered in the light of the general study of ESP to which the dowsing problem clearly belongs. And if, as we have said, the neglect of the problem has been due to difficulties of investigation, then the first step in making the attack should be the improvement and standardization of exploratory techniques. The best and almost the only clear-cut findings thus far are those in which the dowser attempts to distinguish moving from still water in concealed pipes and those made on the location of concealed objects. If we are dealing with ESP we already know enough to be able to design workable tests along those lines that will give us reliable data at a much more rapid rate than anything done in the dowsing field in the past. Methods of independent testing over the same map of a given plot by a number

of different dowsers may be used if the beliefs of the subjects permit the use of this method, and repetitive testing over the same plot of land itself can of course be done safely if all traces can be removed after each trial.

While we must work as a matter of policy within the framework of the dowser's conviction, we can do it selectively, of course, and try to determine which of the methods within that framework operate most successfully for him. The objective should be not so much to test the claims of the dowser but to see to what height of efficiency ESP can be elevated by following all the variations of practice to which the dowsing art has lent itself, even if this should prove to be more costly and time-consuming than the underground pipe or the hidden object method. If the efforts are oriented toward securing the greatest possible reliability in the application of ESP rather than attempting to evaluate the reliability of a folk-way practice we are more likely to keep the problem in the proper focus.

One obstacle to the investigation of dowsing in the past has been made less serious. In general the low rate of success demonstrated by the dowser under test would not encourage the attempt to use dowsing in any practical way. With the aid of statistical instrumentation, however, a relatively small percentage of success above the score expected from chance can be converted into practical certainty if properly handled in application. Equally good practical results can be had from extrachance hitting below expectation. The dowser who would throw away his rod in disgust when successfully locating only three targets out of ten when one target is expected from chance need do no better than that on the average to achieve an effect of complete reliability, provided of course his percentage can be maintained. With this fact in mind the experimenter may be able to stave off better in the future the frustration and upset the dowser tends to experience in discovering that his rod is not always right. In consequence of this and some other new developments in our research field, the current upsurge of interest in dowsing may lead at last to inquiries justifying acceptance, first of course by parapsychology, but eventually by science in general.

J. B. R.

MULTIPLE ASPECT TARGETS IN TESTS OF ESP

By Esther Bond Foster

ABSTRACT: A multiple aspect target is one in which at least two properties or aspects must be known before the target can be identified—such as a playing card, for instance, which can be identified only if suit and number are both known. In the following report data from some of the published ESP experiments involving multiple aspect targets are studied to see whether the subject was getting impressions of the complete card or whether he was getting impressions of the separate aspects and was making a hit when separate correct aspects happened to coincide. The results indicate that the subjects in the data examined obtained an ESP impression of the card as a whole.—Ed.

If a subject is calling an ESP card he names one of the five symbols and his call is completely right or completely wrong. On the other hand, if he is calling a playing card, he names both suit and number, and his call may be completely right, right for suit only, right for number only, or completely wrong. Other possibilities are to score his call for color and for whether or not it is a face card. Thus, such target material as playing cards may be spoken of as "multiple aspect" material. We may define a multiple aspect target as one for which there are at least two characteristics that must be specified in order completely to identify that target. Another example of multiple aspect material is furnished by the checkerboard of numbered and lettered squares used by Heymans, Weinberg, and Brugmans (3) in which the column is one aspect and the row the other. Still another example is numbers greater than nine, which may be spoken of as having the units digit for one aspect, the tens digit for another, and so on.

All multiple aspect material can be analyzed in terms of different probabilities of chance success. In a full deck of playing cards, the chance of getting a complete card hit, that is, suit and number both right, is 1/52. The chance of getting color right is 1/2; that of suit, 1/4; that of number, 1/13. The subject may also score such partial hits as color and number right (suit may be right or wrong) for which his chance is 1/26. Fisher (6) has worked out a formula for evaluating playing card material which

takes into consideration all the ways in which a call can be right or wrong.

It might be helpful in our understanding of the characteristics of ESP to study the subject's success with the different p-values of multiple aspect material. If a positively scoring subject responds to the target as a complete unit, his whole success will be in his complete card hits. In such a case the "ten of clubs," for instance, would be perceived in its entirety rather than as the "ten of something" or the "something of clubs." In contrast, a positively scoring subject may respond to each aspect as independent of the others; that is, his whole success may lie in the separate aspects. In the former case, there will be no further deviation beyond chance on *suit* and *number* in the trials which are not complete card hits. In the second case, there will be no more *complete card* hits than the number which would occur by chance association of the given suit and number hits. This chance association expectation is the product of suit hits and number hits divided by the number of trials. The chance association expectation can be similarly calculated for suit and number both wrong (complete misses), suit right and number wrong, suit wrong and number right. By a chi-square method we can find the probability that the distribution of cases obtained in these four categories is determined by chance association of suit and number.[1] This is the calculation to which reference is made when we speak of the probability that there is chance association.

Responses made exclusively either in terms of the complete target or in terms of its separate aspects may not be the general rule for ESP operation. It may be that in a group of trials there are

[1] For two-aspect targets the hits on the separate aspects, A and B, determine the scores expected in each category as shown in the contingency table:

	B wrong	B right
A wrong	A wrong and B wrong	A wrong and B right
A right	A right and B wrong	A right and B right

some trials in which ESP conditions are so good that the subject is very likely to get the complete card, other trials where conditions are fair for each aspect, and other trials in which chance alone operates. The subject will then have more hits on complete card than expected by chance association of suit and number, but he will also have some deviation on suit and number after complete card deviation is subtracted from the score of each. Suppose, for example, that the subject is reacting to the aspects separately but that the same position effect pattern obtains for both, such as a U-curve of scoring. This situation would tend to give him an excess of complete card hits over those expected by chance association of suit and number. Any factor which tends to yield success on one aspect at the same time as on the other will tend to increase the complete card hits above the number given by chance association. The principle involved may be illustrated by a simple social situation. If two individuals, A and B, are in the habit of going to the same places, they will tend to be seen together. (Seeing A and B together is analogous to a complete hit.)

The above remarks were made with a subject who is scoring positively in mind. Sometimes a subject tries to avoid the target (negative aim), and at other times his deviation is negative even though his avowed conscious aim is to "hit" the target. The same simple algebraic deductions as form the basis for the description of the positively scoring situation could be applied to the negatively scoring situation. If ESP is sometimes positive and sometimes negative but the factors favoring success on the suit and number vary together (that is, the subject tends to "hit" them both on one trial and to avoid them both on another) the score on complete card hits tends to be more than its chance association expectation. The complete card hit score may be a sensitive measure of covariation at the level of the individual trial.

It is theoretically possible that a subject might have different attitudes toward the different aspects of a multiple aspect target and that these attitudes would result in positive scoring for one aspect and negative scoring for another. The possible experimental situation which would result from this attitudinal difference would give some interesting results. The writer has done some experimenta-

tion along this line, but the project is only begun and it would be premature to present the few suggestions obtained. In most of the experimentation conducted by others the avowed aim of the experiment has been to score positively on all aspects and the deviations obtained have been chance or positive. Let us now examine some of these experiments that have been conducted with multiple aspect material.

Some Experiments Using Multiple Aspect Material

In some of the experiments using multiple aspect material there has been very little evidence of ESP. This group includes experiments by Thomas (**11**); Besterman, Soal, and Jephson (**2**); and Murphy and Taves (**7**). In the last-named experiment there was evidence of a covariation between scores on runs made with playing cards and scores on runs made with other material, but there was only chance scoring on the playing card material considered alone. In other experiments, some of which are described below, the success has ranged from slight to striking.

In 1888 and 1889 Richet (**8**) in France, and Professor and Mrs. Henry Sidgwick and Mr. G. A. Smith (**9**) in England conducted some experiments with multiple aspect material with striking success. Richet used a playing card enclosed in a commercial opaque envelope for each clairvoyance trial with his subject, Léonie, under deep hypnosis. He made three similar experiments, of which the second, the one discussed here, was the only successful one. The Sidgwicks and Smith used numbers from 1–90 for GESP trials with two hypnotized subjects. No detailed analysis of these experiments will be given because their conditions were not entirely above criticism, and if there had been any sensory cues these would have tended to give results in some ways favoring the conclusion to be drawn from all multiple aspect material surveyed. In both experiments the most striking success was on the complete card, but the manner of the subject's giving his response was fragmented. Léonie decided on suit first and after that spent considerable time counting over and over on her fingers to decide on number. The subjects of the Sidgwicks and Smith often "saw" the digits appear one at a time at intervals of the order of half a minute.

With Léonie the complete card success is most remarkable. She made a total of 68 trials, but on most of them she designated only the suit since she failed to get an impression of the number of the card. In the 51 trials on which she designated only suit she made 21 hits (dev. = + 8.2; P = .017). On two trials she said "face card" of a particular suit, and these were both hits as far as they went. In the 15 trials in which she designated both suit and number there were 12 complete card hits where the P of 6 hits or more by chance is .000,001. On the other three trials she got suit right once and number right twice.

With the subjects of the Sidgwicks and Smith there is a significant deviation from chance on the separate digits of numbers which were not right in both digits, but the critical ratio of these hits is much less than that for complete number hits. The two subjects made a total of about 98 complete number hits, whereas chance would have given them about eight hits. On the trials which were not successful for complete number, the units or tens digit was right 159 times, whereas chance would have given 116 hits. The deviations from chance on complete number and on separate digit are both significant, but the deviation on complete number is greater. Chance association of digit hits would give an expectation of about 47 complete number hits. The distribution of cases is obviously non-chance, that is, the hits on the separate aspects are not independent. On some days the subjects of the Sidgwicks and Smith were less successful than on others in getting complete number hits, although the total complete number hits for the less successful days is still significantly more than chance (246 trials, 12 hits). On these same days the total score for separate digits was at chance level (46 obtained, 44 expected). These results seem to indicate that the complete number score is the most sensitive indicator of ESP. It is interesting that in both experiments the deviation from chance occurred principally on the complete card although the separate aspects were called by the subjects at sometimes widely spaced intervals.

John E. Coover's 1917 experiments (**4**) were conducted with playing cards from which the face cards had been removed. His main experiment was carried out with one hundred students, who

did 5,135 trials of GESP and 4,865 trials of BT, and ten "psychics," who did 530 trials of GESP and 470 trials of BT. He considered that the BT procedure gave control trials, although he does seem to have conceived the possibility that clairvoyance might take place. Coover said he would consider his results positive if they met "the requirement of a degree of accuracy usual in scientific work," which he took to be represented by P = .000,022,1. When his results fell short of this high standard he did not consider them to be positive. In the GESP trials of all subjects there were 171 complete card hits in 5,665 trials which gives a deviation from chance of +29.4 (CR = 2.50, P = .012), a figure which is generally regarded as highly suggestive. Since we now know that ESP is likely to function in the BT condition which Coover regarded as a control, and since these trials are positive, though insignificant, we should pool Coover's experimental and control trials to study total ESP effects. This gives us 11,000 trials with 323 complete card hits, which gives a deviation from chance of +48 (CR = 2.93, P = .0034). If we consider suit separately, we have 2,864 hits with a deviation of +114 (CR = 2.51, P = .012). The number of complete card hits expected from chance association of hits on suit and number is 293. Coover did several secondary analyses, which yielded only chance results. In Coover's experiment, then, the strongest evidence of ESP is given by the complete card hits, though the suit hits considered separately are significant at the one per cent level. The probability that there was only chance association of suit and number hits is .032.

In the years 1917-1930 Dr. P. A. Dietz (**5**) conducted 4,712 trials of clairvoyance with playing cards. He and others served as subjects. When he was subject himself, he enclosed a card which he had not seen in an opaque envelope or put it between the pages of a book. The total deviation from chance for all subjects was: color, +77 (CR = 2.24, P = .025); suit, +170 (CR = 5.72, P = 1.2×10^{-8}); number, +40 (CR = 2.18, P = .029); and complete card, +37.2 (CR = 3.94, P = .000,081). The chance association of the given suit and number hits would yield an expectation for complete card hits of 115. The difference between the number of complete card hits obtained and those expected from

chance association of suit and number hits is positive but not significant. It would seem in this experiment, then, that the ESP impression of suit was the strongest contributor to the total success. It is interesting to note that the number of hits on color is not greater than chance when suit hits are taken into account. The subjects seemed to get an impression of the suit directly rather than to perceive the color first and then to perceive which of the two suits of that color was the one belonging to the target.

In 1919 Heymans, Weinberg, and Brugmans (3) in Holland carried out a strikingly successful GESP experiment using a checkerboard of eight letters horizontally and six numbers vertically as target material. There were thus 48 letter-number combinations, one of which was randomly[2] chosen for each trial. The blindfolded subject tried to put his finger on the letter-number combination to which the agent tried telepathically to direct him. In 187 trials there were 60 complete hits as compared with a chance expectation of 4. The chance probability of 17 or more complete hits is .000,001. The experimenters studied the responses made in the 127 trials which were not complete hits. Their analysis is done in terms of the average number of cells between response letter and target letter and the average number of cells between the response number and target number. We would refer to this type of analysis as one pooling direct and displacement hits. The chance expected average distance for the letters is 2.63 cells, whereas the average distance of the subject's response position was 1.91 cells. The subject came 0.72 cells nearer to the target than chance would give, a deviation which has a CR of 5.0 ($P = 5.73 \times 10^{-7}$). The chance expected average distance for the numbers is 1.94 cells, whereas the average distance of the subject's response position was 1.28 cells. The subject came 0.66 cells nearer the target than chance would give, a deviation which has a CR of 5.3 ($P = 1.16 \times 10^{-7}$). We can infer from Brugman's report that there were about 102 hits on numbers and 92 hits on letters. This would give deviations of +71 for numbers, +69 for letters, and +56 for complete hits. There is, then, highly significant evidence of ESP in the calls which were not complete hits, though the deviation from chance of the complete hits is more strik-

[2] The targets did not turn out to be random, but their non-randomness was not such as to change the number of hits expected by chance.

ing. The chance association of letter hits and number hits would give an expectation of 50 complete hits. The probability that there was only chance association of the letter and number hits is .0041. The experimenters tell us that the number of complete hits was greater for some experimental conditions than for others. If there were also more letter and number hits in the trials that were not complete hits, this situation alone would give some varying together, that is, more complete hits than the expected number calculated from the pooled data for all experimental conditions. We do not have the information at hand to evaluate the conditions separately. In this experiment, then, there is highly significant evidence of ESP in the scores of the complete hits and also in the scores on the separate aspects of calls that were not complete hits. There are probably more complete hits than the chance association of letter and number hits would give.

The procedure used in the most recent multiple aspect experiment differed from that of the preceding experiments in that the subject called an uninterrupted series of 25 cards at the rate of about three seconds per call rather than calling the cards one by one at much more widely spaced intervals. Dr. S. G. Soal conducted 80 runs of 25 calls using the GESP procedure with Mrs. Stewart as subject.[3] Her deviation on color is +24; on suit, +16; on number, +22; on complete card, +39. The critical ratio of the card hit deviation is 6.3 ($P < 2 \times 10^{-9}$). There is no deviation on any separate aspect that is greater than the card hit deviation. Dr. Soal's interpretation of this result is that Mrs. Stewart reacted to the complete card. This interpretation seems the simplest for this case. An alternative interpretation might be that there was a strong tendency toward covariation of the separate aspects but this interpretation is not borne out by the analyses I have made.

These 80 runs with playing cards were alternated with 80 runs with cards showing the initial letter of one of five animal names (**10**). The average scoring rate on the latter where the probability is 1/5 and the expected average is 5.0, is 6.9 hits per run (CR = 7.4), and these runs contain two striking secondary effects which are

[3] Dr. Soal has not yet published the data with playing cards, although he has calculated the main result reported here. He has kindly given the writer permission to make and publish further analyses.

being reported by Soal and Pratt. The playing card data can be analyzed for one of these effects which has not yet been published. In the playing card material the complete card data are closely similar to the animal names data in this effect, but color, suit, and number data considered separately when the complete card was not right show no significant effect.

The other striking secondary effect shown by the letter material is a tendency to score below chance on card sequences which have no change in target (doubles, triples, etc.). That is, if the same letter occurred as the target two or more times in succession, Mrs. Stewart did more poorly on these trials than on the sequences in which the target letter changed. In the playing card material there are so few unchanged sequences of complete card that they are not amenable to statistical treatment. Similar analyses for suit and number showed no significant effect.

In Mrs. Stewart's playing card data there were twice as many complete card hits as would be given by chance. There was no further deviation on color, suit, or number after the complete card hits were taken into account. The complete card hits can be analyzed for one of the secondary effects shown by the alternating letter material, and they show this effect significantly. The color, suit and number hits can be analyzed for both the secondary effects shown by the alternating material, and all of these aspects show neither effect. In both the primary and the secondary manifestations of ESP, then, there is significant evidence in the complete card hits and no discovered evidence in the hits on color, suit, and number considered separately after complete card hits have been taken into account.

Summary and Conclusions

The results of all the experiments reviewed are summarized in Table 1. In all the experiments the number of hits on complete card is greater than the expectation from chance association of hits on the separate aspects. This result is statistically significant for some of the experiments. In only one experiment, that by Dietz, does the principal success seem to be on a separate aspect rather than on the card as a whole. In the Coover experiment the strongest suggestion of ESP is on the complete card, and in the Sidgwick

and Smith experiment on the days when the complete hit score was not so high as on other days, the separate aspect scores are chance. In the Heymans, Weinberg, and Brugmans experiment there is highly significant evidence of ESP in both the complete hit score and the separate aspect scores. The complete hit score is probably greater than chance association of the separate aspects would give. In the Soal experiment there is a highly significant indication of ESP in the complete card hits, while there is no noted primary or secondary evidence of ESP in the separate aspects.

On the basis of the facts now available, it is not possible to place

Table 1

SUMMARY OF RESULTS*

Experimenter	Separate Aspect Hits							Complete Card Hits			Probability that there is only chance association of separate aspects
	Trials	p_1	Hits	Dev.	p_2	Hits	Dev.	p	Hits	Deviation from chance	
Sidgwicks & Smith	669	1/9	185	+111	1/10	170	+100	1/90	98	+90	$<10^{-6}$
Coover	11,000	1/4	2,864	+114	1/10	1,126	+ 26	1/40	323	+48	.032
Dietz	4,712	1/4	1,348	+170	1/13	402	+ 40	1/52	128	+37	.13
Heymans, Weinberg, & Brugmans	187	1/6	102	+ 71	1/8	92	+ 69	1/48	60	+56	.004
Soal	2,000	1/4	516	+ 16	1/13	176	+ 22	1/52	78	+39	$<10^{-6}$

*Richet's data cannot be included in this table because his subject did not give a complete response on each trial.

a single interpretation upon the findings of all these experiments. The results of the Soal experiment do seem to indicate that a subject can respond unitarily to the complete card without getting any impression of the separate aspects. On the other hand, the results of the Dietz experiment would seem to indicate that a subject can respond to one aspect more successfully than to other aspects. To the degree that the chi-square method used is a general test of the varying-together of the separate aspects, all of the experiments reviewed showed that the aspects varied together. Since all of the experiments except that of Soal employed more than one subject or more than one condition, we cannot definitely say that there was any varying-together other than the finding of more ESP in some subjects or some conditions than in others. It would seem, then, that in the experiments so far conducted the findings from the Soal data

indicate that the subject tended to obtain an ESP impression of the card as a whole and that the other multiple aspect experiments either showed the same effect or that in them the opportunities to obtain ESP impressions of the several aspects varied together. Further light on multiple aspect targets will have to await the acquisition of data obtained under uniform conditions.

Beyond its theoretical significance, the comparison of success on the complete target with that on the aspects may have brought to light a fact of practical importance to research workers. In the Coover experiment, where the evidence of ESP is minimal, the complete card hit gives the highest critical ratio. In the experiment of the Sidgwicks and Smith on the days when the complete number score was relatively low, though still significantly above chance, there was no significant deviation from chance on the hits on the separate digits. It would seem, then, that the complete hit score is a more sensitive measure of ESP than are the scores on the separate aspects.

References

1. Besterman, T. Review of experiments in clairvoyance by P. A. Dietz in *Tijdschrift voor Parapsychologie*, November 1928-September 1929. *J. Soc. psych. Res.*, 1929, **25,** 183-84.
2. Besterman, T., Soal, S. G., and Jephson, I. Report of a series of experiments in clairvoyance conducted at a distance under approximately fraud-proof conditions. *Proc. Soc. psych. Res.*, 1930-31, **39,** 375-414.
3. Brugmans, H. J. F. W. Une communication sur des expériences télépathiques au laboratoire de psychologie à Groningue faites par M. Heymans, Docteur Weinberg et Docteur H. J. F. W. Brugmans. *Compte Rendu Officiel du Premier Congrès International des Recherches Psychiques*, 1922, 396-408.
4. Coover, J. E. *Experiments in Psychical Research at Leland Stanford Junior University.* Stanford University: Stanford University Press, 1917.
5. Dietz, P. A. (See Besterman, T. Review of experiments in clairvoyance by P. A. Dietz.)
6. Fisher, R. A. A method of scoring coincidences in tests with playing cards. *Proc. Soc. psych. Res.*, 1924, **34,** 181-85.
7. Murphy, G., and Taves, E. Covariance methods in the comparison of extrasensory tasks. *J. Parapsychol.*, 1939, **3,** 38-79.

8. RICHET, C. Further experiments in hypnotic lucidity or clairvoyance. *Proc. Soc. psych. Res.*, 1889-90, **6,** 66-83.
9. SIDGWICK, PROF. AND MRS. H.; SMITH, G. A. Experiments in thought-transference. *Proc. Soc. psych. Res.*, 1889-90, **6,** 128-70.
10. SOAL, S. G., AND PRATT, J. G. ESP performance and target sequence. *J. Parapsychol.*, 1951, **15,** 192-215.
11. THOMAS, N. W. *Thought Transference.* London: Alexander Moring, Ltd., 1905.

Parapsychology Laboratory
Duke University
Durham, North Carolina

THOUGHT TRANSFERENCE AND RELATED PHENOMENA[1]

By R. H. Thouless

The topic I shall be discussing tonight is now often called Experimental Parapsychology. The word "parapsychology" is roughly equivalent to what has been called in this country "psychical research." I am not proposing to discuss the more dramatic and entertaining side of psychical research which includes poltergeists, hauntings and communications with departed spirits. It is not that these things are not both interesting and important, but they are not the aspects of psychical research in which my own interests have centered. What I want to do tonight is to give some account of the attempt that has been made to unravel this complicated field of unexpected occurrences by the method of experiment.

Interest in thought transference started, as many scientific interests have started, with anecdotes. The kind of thing reported was that X had an impression that some misfortune had happened to his friend Y, at the same time as Y, in some other place, had truly suffered a misfortune which could not have been known to X by any normal means of communication.

Such stories are in general of little value as evidence. If the correspondence between X's impression and the event happening to Y is exact and detailed, the explanation of the coincidence as a result of chance becomes a very improbable one. More generally the coincidences have not been exact or detailed and, although such occurrences may seem impressive to the person experiencing them, there is no way of deciding how likely it is that they are really due to accident.

The experimental problem was to produce a laboratory situation in which the same type of coincidence might be observed in events for which the chance expectations could be exactly assessed. Experiments have been of various kinds. Sometimes the experimenter has drawn a picture while the subject of the experiment, in some

[1] This article is taken from an address delivered by Dr. Thouless before the Royal Institution of Great Britain on Dec. 1, 1950. It is reprinted here from the *Proceedings of the Royal Institution* with their permission and that of the author.

other room, made an attempt at reproduction of that picture. Experiments of this type were carried out in the Society for Psychical Research as early as 1884 and more recently by Warcollier (21) and Whately Carington (2). The early experiments showed some striking resemblances between the target picture and that produced by the subject, but the science of statistical methods had not at that time been sufficiently developed for the experimenter to make an exact estimate of the evidential value of these resemblances. The problem of exact statistical treatment of such reproductions of drawings was dealt with in the recent work by Whately Carington but it remains difficult and of somewhat uncertain rigidity.

Most experimenters have preferred to follow the easier path of using cards as their material. In the early days, ordinary packs of playing cards were used. More recently Professor Rhine of Duke University introduced a pack of 25 cards having on them five types of diagram, a cross, a circle, a star, a square, and waves (9). In the first few years of the Society for Psychical Research, successes were obtained in card guessing experiments. Although methods for adequate statistical treatment of these results were at that time unknown they are generally sufficiently reported for it to be possible to say with confidence that successes significantly exceeded mean chance expectation and that evidence of some form of paranormal cognition was adequate.

That this result was considered at that time to be evidence of telepathy or thought transference was the natural consequence of the condition under which the experiments took place. The kind of experiment that was done was one in which the experimenter turned over the cards one by one and looked at them while the subject, with his back turned to the experimenter or in another room, called out the name of the card which he thought the experimenter had turned up. That there was insufficient logical grounds for the conclusion that success in these experiments was due to thought transference is clear from the fact that there is nothing in the experimental situation to exclude the possibility that it was not the agent's knowledge that a certain card had been turned up but the fact itself that was the determining condition for the subject's correct responses. This was commonly overlooked and such experiments were regarded as proving the fact of thought transference.

This overhasty interpretation led to no serious error in fact since later research has shown that knowledge in the agent's mind of the right response is a sufficient condition of success in parapsychological experiments although it is not a necessary one since success may also take place in its absence.

The modern period in parapsychological experimentation started in 1927 when Dr. J. B. Rhine joined McDougall's psychological department at Duke University in North Carolina, and started a new attack on the experimental problems. Professor Rhine certainly succeeded in bringing experimental parapsychology into the limelight, although the attention he aroused was sometimes critical. Rhine has paid generous tribute to the work of the early psychical researchers and in his first book he deplored the fact that every new worker in this field was expected to prove the basic facts all over again (**9**). The parapsychological laboratory at Duke has not been the only center of research since 1927; the new interest in parapsychological experimentation has led to fruitful work in this country and other European countries and in other American universities.

Whereas most of the early experimenting had been done with a very small number of exceptional individuals who scored at a high rate, Professor Rhine and his colleagues showed that it was also possible to get parapsychological successes at a lower rate with unselected groups of ordinary people. There is an obvious experimental convenience in finding a high scoring subject and experimenting with him. At the same time, there are certain problems, such as that of the relationship between parapsychological success and personality traits, which can only be dealt with experimentally by using large groups of ordinary people. There is obviously room for both types of experimentation.

During the period here under review, both Mr. Tyrrell and Dr. Soal have carried out important and fruitful researches with high-scoring subjects (**20, 14, 13**) while Whately Carington contributed an important group experiment on the reproduction of drawings (**2**). I have myself experimented with one high-scoring subject and I am carrying out one experiment with an unselected group. Most of my own work has, however, been with myself as subject (**16, 15**). This also is a method of experimenting deserving atten-

tion from parapsychological experimenters. In particular, it has the merit of being the most hopeful approach to the important practical problem (at present unsolved) of whether we can devise means by which the exercise of these capacities can be brought under willed control.

A further new advance started at Duke University was the attempt to devise experiments for discriminating between what were called "telepathy" and "clairvoyance," that is, between paranormal success under conditions in which what is reacted to is a fact known to some other person (telepathy) and when it is a fact not known to any other person (clairvoyance). The first experiments in paranormal cognition of an event not known to any other person were by means of what Rhine called the "down-through" technique. In this experiment, instead of the experimenter looking at the cards, he placed the pack face downwards on the table and asked the subject to guess the order of all twenty-five cards before any card was turned up. This obviously eliminated the possibility that the subject's right responses were determined by knowledge in the experimenter's mind since the experimenter did not know the order of the cards any more than did the subject. In this condition, success was obtained not noticeably, if at all, inferior to that obtained under the conditions in which the experimenter did look at the card.

This down-through technique remains a useful method of experimenting. It was not later considered by Rhine to exclude the possibility of telepathy since it might be that the subject was precognizing the knowledge that would be in the experimenter's mind when the check was made a few minutes later. Consequently a new technique of experimenting was devised in which the cards were put into five heaps corresponding to the five symbols and each heap was shuffled before the check. Since, under these conditions, no one could ever know which particular guesses were right and which were wrong, it was considered that explanation by precognition of the check was ruled out, and the reality of "clairvoyance" was established (**10**).

It was more difficult to devise an experimental situation in which knowledge in the experimenter's mind was the only condition which could determine success since there was no experimental event such as the position of a card in a pack. The earliest attempt was an

experiment in which the experimenter merely thought of a series of symbols. This, however, is a form of experiment for which there is no rigid method of assessment since the statistical treatment of card guessing results must assume that one of the two card orders compared is a random one. Its use is erroneous if both series are produced by human thought, since in this condition we know that neither series can be random and we have no means of knowing how far departures from randomness are likely to run parallel in both series. A number of workers suggested an improved experiment in which the order of the experimenter's thought would be externally determined by the use of a pack of cards with a code for translating the observed card into a symbol in his mind, which code would not itself be written down. Successful experiments under this condition of "pure telepathy" have been done by Dr. Soal (**13**) in this country and by Miss Elizabeth McMahan (**5**) at the Duke University Parapsychology Laboratory. It seemed, therefore, reasonably well established that success in card guessing experiments could take place both under "pure clairvoyance" conditions when the order of the target pack was not known to anyone at any time and under "pure telepathy" conditions when the target had no external existence but was only a thought in someone else's mind.

There is not universal agreement on the first point. In the Soal-Goldney experiment with Basil Shackleton (**14**) it is reported that he succeeded only under conditions in which the agent knew what the target card was and this is often advanced as an argument against the reality of "pure clairvoyance." For many reasons, I do not find this argument a very forcible one. The failure of one experimenter to confirm the findings of another does not prove that that other is wrong; least of all in a field where the relevant factors are so little understood as here. Also the experiment in which Shackleton failed was neither the same as Rhine's "down-through" experiment nor the same as his "pure clairvoyance" experiment, since in the Soal-Goldney experiment, unlike the Duke experiments, the order of the target pack (of random numbers) was known to the agent; what was unknown was the coding system by which these numbers were to be translated into the five symbols guessed by the percipient. I do not suppose that this difference was important in determining success or failure, but it may have been. A more se-

rious point is that of the ten experiments done under these conditions, Shackleton on one occasion achieved the very considerable feat of making 58 correct guesses out of 200. This excess of 18 over mean chance expectation is one that by chance alone would not be equalled or exceeded more than once in about 700 times. When we take into account the fact that this was the result of the selected best of ten experiments it cannot be regarded as very convincing evidence that Shackleton could succeed under these conditions, but it remains sufficiently unlikely to be a chance result to make one unwilling to assert with any conviction that he could not succeed under "clairvoyance" conditions. It seems a pity that Dr. Soal did not do more experiments of this type with the same subject to see whether this apparent success would be repeated.

To return to the new contributions to parapsychology made during recent years. One must place to the credit of Professor Rhine the devising and carrying out of the first card test for precognition (8). This involved guessing, not the order of the cards at the time of the experiment but some order which they would have in the future. There were several changes in method of experimenting to meet various objections, so I will describe the experiment as it is most commonly done now. A pack of ESP cards is taken and the subject is told that he is to guess what the order of the pack is going to be after it has been subjected to a process of shuffling and a random cut. He then makes his guesses, which are recorded. At some later time, which may be immediately afterwards or a week or more afterwards, the experimenter shuffles the cards. He then cuts them at a randomly determined point by some such method as the following. He throws three dice marked A, B and C and the uppermost face of A tells him which page of a table of random numbers he is to turn to, that of B tells him which column he is to select, and that of C which row. Following these directions he finds a random number, let us say 67, he takes the remainder of this number after division by 25 (i.e., 17) and cuts the pack below the seventeenth card.

This somewhat elaborate system of precautions is to ensure that the experimenter cannot by any means, conscious or unconscious, make the pack correspond to the guesses already given, either in the shuffling or in the choice of a point at which they are to be cut. This

aim is completely achieved by the random cut and its fulfilment does not depend in any way on the character of the shuffling. This follows from the fact that there are twenty-five points at which the cut could take place (including the possibility of no cut at all), and the average score that any system of guesses would give when checked against the pack as cut in all twenty-five of these ways must, by mathematical necessity, be exactly five. This would obviously be true even if, before the cut, the pack had been shuffled into exactly the same order as the subject's system of guesses.

I think most people feel more disturbed at the possibility of precognition than at thought transference even though they may find it difficult to say clearly why it is so unexpected. Nevertheless we must disregard the protests of common sense; experiments under these conditions do also work. I have myself obtained success at a satisfactory although not startling rate under the conditions I have just described. Some degree of determination of guessing by an event which lies in the future seems adequately proved.

If this finding seems to conflict with common sense expectations, one can only say "So much the worse for common sense expectations." If what is found to happen does not conform to our expectations, we must try to make our expectations conform to what is found to happen. I cannot suggest how that is to be done; it is a problem for the future.

I should like here to interrupt my account of what has been found out to consider the question of how we ought to talk about it. Always our choice of words is liable to imply a theory, and it is necessary to examine the implications of our scientific language to see whether we really want to saddle ourselves with the theories it implies. Parapsychology has inherited a curious polyglot terminology ("thought transference" or "telepathy," "clairvoyance," "precognition"). The use of these terms seems to imply more than it is safe to assume. When we talk of thought transference we are implying that success in card guessing experiments (under the conditions in which the experimenter or agent knows the card) is due to the transference of a thought from his mind to the subject's. This may be the right way of describing what happens, but it may not; it goes, at any rate, far beyond a description of what is actually observed in the experiment. The subject calls out or writes down a

symbol; his series of guessed symbols shows a correspondence with the actual order of the pack beyond what we should expect from chance. The subject reports no thought coming into his mind labelled with the character of having been originated in the agent's mind; indeed he generally reports no thought at all, he just carries out the action of calling out or writing down the symbols.

Also the use of the terms "thought transference," "clairvoyance," and "precognition," implies that we are dealing with three kinds of paranormal occurrence. Of course, there may be three or more, but nothing in our observations suggests that this is the case, rather than the opposite possibility that there is one paranormal process which may occur when someone else knows the card, which also may occur when no one knows the card, and whose occurrence does not depend on whether the card order reported is a present fact or a future one. Considerations of this kind led Rhine to use "extra-sensory perception" (or ESP) as a portmanteau term to cover the process of successful guessing under all these conditions (**9**). Others have used "paranormal cognition" in the same sense. These too are open to the objection that they imply a theory; the first that the process concerned is a kind of perceiving, the second that it is a kind of knowing. Neither is justified by the experimental facts; the successful subject does not perceive anything and he does not know even whether a particular guess is right or wrong.

Dr. Wiesner and I have, therefore, suggested that the process should be indicated by a mere symbol which would carry no implications as to the nature of the process and no implication as to whether we are dealing with one process or many. For this purpose, we have suggested as a symbol the Greek letter Ψ (**17**). This suggestion has now been very generally followed, although those who talk of "psi processes" do not always follow the implications of our choice of a meaningless symbol. One of these implications is that we wish to outlaw the traditional controversies as to whether success under particular conditions is "due to thought transference or clairvoyance" and to replace them by the experimentally soluble problem of what are the necessary or sufficient conditions of psi success (i.e., such conditions as presence or absence of knowledge in someone else's mind, present or future existence of that knowledge or of the event to which response is made, etc.).

It is not, of course, our wish to stop people from ever using the traditional terms. It is sometimes more convenient to speak of "thought transference" than to use the more exact but awkward phrase "a psi process under the condition that the agent knows the fact that the subject is reporting." Indeed when we are speaking loosely, we use the old terms ourselves. We do, however, want to discourage the habit of using them as if they were adequate descriptive terms, and to avoid the danger that their implications may become part of the accepted and unexamined structure of our thought.

Another finding very early reported from Rhine's laboratory was what he called "chronological decline" (9). It was found that if a subject who was guessing right more often than could be accounted for by chance went on sufficiently long, his rate of scoring fell off and in many cases (especially if he were driven to go on experimenting when he did not want to), he actually started scoring less than one would expect by chance. When this was first reported, it struck me as very odd but it has been fully confirmed by later research. It is, of course, obvious that consistent scoring below mean chance expectation requires in some sense paranormal knowledge of the true card order no less than scoring above mean chance expectation. There is no great mystery as to how this can happen since it would always happen if the subject knew what the card was and systematically replaced one symbol by another. While negative scoring is itself a problem of considerable interest, it is likely to strike the investigator first as an impediment to his research since, when it is his primary aim to produce an overall positive deviation as evidence for the reality of psi, if some of his subjects are scoring negatively they will tend to dilute the evidence obtained by those who are scoring positively.

The workers at Duke University found, not only this tendency to a general decline of score through a series of experiments with the same subject, but also significant variations in the rate of scoring at different points in the same experiment (11). It is interesting that odd effects should be found by different workers, experimenting independently of one another. They confirm the impression that we are travelling through a country that has features even if we do not always understand them.

A similar odd and unexpected finding, first reported in this

country, was the phenomena of "displacement," first noticed by Whately Carington (2) and afterwards amply confirmed by Soal (14). This was the fact that, when the subject was failing in the task of guessing the target he was intended to guess, he might be succeeding in guessing some other target (in Soal's experiments it was the card one ahead of the one turned up by the experimenter). In experiments on card-guessing with myself as subject and my wife as agent, I found significant displacement to the card two ahead of the one turned up (16). Although displacement had not been observed at this time in the American experiments, a later study of some of the American results showed that displacement was also taking place in these (1).

An aspect of psi phenomena that has attracted much attention recently in American laboratories (both at Duke University and at the City College of New York) is the correlation between psi success and measurable qualities of personality. An example of this work is the finding by Dr. Betty Humphrey that, if subjects are classed by a test on drawing as compressives or expansives and are also given a test in parapsychology, then the compressives tend to score positively under telepathic conditions, but below chance expectation under clairvoyant conditions, whereas this relationship is reversed for those ranked as expansives (4). I think this is particularly interesting as showing the difficulty of getting positive results in a group of people not selected for their paranormal ability. In Dr. Humphrey's groups the overall score does not differ significantly from chance for the whole group although, when the group is split up by means of a psychological test which is not in itself a test of psi ability, it is found to consist of a sub-group of individuals scoring positively and another sub-group scoring negatively. Unless that discrimination into the personality types of compressives and expansives had been made, the group as a whole would have appeared to be showing no psi ability at all.

One of the most startling new lines of work developed in the Duke parapsychological laboratory was on what Rhine has called "psychokinesis" (PK) which he picturesquely describes as "the action of mind on matter" (12). This too had an earlier history in the field of observation and anecdote. Some mediums have been reported to have been able to move pendulums and other physical

objects, and the movements of physical objects without contact is a characteristic feature of the reports of poltergeist hauntings. The novelty in the Duke work was the devising of a practical experimental technique in which a number of dice were thrown while the subject had the intention that a certain face should fall uppermost.

The early experiments were in some respects defective. The subject was very commonly allowed to select for himself what the target face was to be and that left open the possibility that success was due to his precognition of the dice falls and not to any influence of his intentions on the dice. This imperfection can be overcome by using some random method of dictating targets to the subject, as for example, by use of a Latin Square arrangement of targets as standardized in biological experiments by Professor Fisher (3). Also in many of the early experiments insufficient account was taken of the fact that most commercial dice are biassed and that in no case can dice be safely assumed to be unbiassed. It is, however, easy to arrange the design of the experiment in such a way that the bias of the dice will not affect the mean chance expectation of success. This can most easily be done by arranging that each face is target for an equal number of times. The mean chance expectation of success will then be exactly six whatever may be the bias of the dice.

Another objection that may be made against any experiment to demonstrate a paranormal effect of the subject's intention on dice falls is that it may be a result of an unconscious skill in throwing. Results, however, continue to be successful when the throwing is completely mechanized, and indeed seem to succeed better under those conditions than with hand throwing. If it is objected that the fall of the dice may be normally determined by the way in which they are initially introduced into the machine throwing them, this objection may easily be overcome by arranging the dice always in a prescribed order with the faces in the same initial position.

It is not infrequently said that PK cannot be demonstrated on this side of the Atlantic. This is an exaggeration. Unsuccessful attempts at the repetition of the PK experiments have certainly been reported in this country but several workers also have had successful results. Of this number, I happen to be one. I have carried out PK experiments myself, taking all the precautions I have suggested above (randomized order of targets, mechanized throwing, and

standardized initial positions of the dice on the machine), and I have got positive results (18). The excess of successes over mean chance expectation was such as would occur by chance only once in 200 times. This is, I think, a sufficient level of significance to make this an adequate confirmatory experiment, although I should not be inclined to claim much strength for it if it were an isolated result which had to bear the weight of a new and unexpected fact.

I have already criticized the underlying implications of such terms as "thought transference" and "extrasensory perception." "Psychokinesis" seems open to the same objection of carrying implications as to the nature of the process involved. It means "movement by mind" and implies a picture of a mind giving the dice a nicely calculated push in order to make them fall the intended way. This may be the right way to think of the matter but it goes far beyond anything we observe in the experimental situation. What we have is a succession of target faces (i.e. the faces the subject is intending to hit), and a succession of dice falls, and a statistically significant correspondence between these two successions beyond what we could reasonably expect by chance. Of what determines this correspondence we know very little. Presumably it is not the mere physical fact that the dice and the numbers representing the targets are in the same room, so it is reasonable to suppose that the correspondence has something to do with someone's mind. But whether it is the subject's mind or the experimenter's, whether the effective condition is someone wanting the dice to fall that way, or expecting them to fall that way, or the mere fact that they are ultimately going to be checked in that way; on all these points we have no idea of the answer.

Again, to avoid unwanted implications in our language, Wiesner and I have suggested using a mere symbol without meaning or implications, and we have suggested that the symbol Ψ_κ should be used for this range of phenomena while Ψ_γ should be used for the thought transference, clairvoyance, precognition range (19). So far as we know, this suggestion has been adopted by no one else.

I have so far been trying to give a factual account of what has happened in experimental parapsychology without arguing as to its soundness. I suppose that we all approach this work first (as I certainly did myself) with deep skepticism. Although skepticism

about these results is still fairly widespread I do not think it is any longer justified. Very often it is based on objections urged many years ago before modern methods of experimenting had been developed. I think four main grounds have been urged for doubt: first, the suggestion of unconscious whispering by the experimenter or agent, secondly that the apparent successes were due to incompetent experimenting, thirdly that they were all due to chance, and fourthly that they were due to a selection of a small number of successful results from a much larger number of unsuccessful ones.

The first suggestion, of unconscious whispering by the agent or experimenter, was a natural way of explaining early results since these were mostly obtained when the agent and the subject of the experiment were close together and often in the same room. It has obviously no relevance to experiments in which the experimenter does not know the card (as in Rhine's DT technique) (**9**), or to results in which the subject is predicting the future order of a pack of cards (**8**), or to those in which the subject is in another town or another country (**13**) (**6**). Since we have abundant evidence that success can be obtained in all these conditions just as well as if the subject is in the same room as the experimenter who knows what is the right answer, the hypothesis of unconscious whispering is no longer a possible explanation of the results.

The second charge of incompetent experimentation is again one which dates from earlier days. It is certainly true that many of the exploratory experiments done in the early days at Duke University were insufficiently safeguarded against the possibility that the subject might be getting his information by other than paranormal means and it is arguable that in the initial reporting insufficient care was taken to distinguish between these experiments and those which could properly be regarded as evidential. The most modern experiments have, however, been surrounded by every experimental precaution that has been suggested by the critics of parapsychology or devised by the experimenters themselves. Two examples of strikingly successful experiments with a most elaborate system of experimental precautions are the Pratt-Woodruff experiment at Duke University (**7**) and the Soal-Goldney experiment in London (**14**). I think indeed that one can claim that the reality of

the results has been more adequately safeguarded than any other doubtful observation in the history of science.

Can the results then be explained in the third way as due to lucky accident? There is an important difference between the situation in the early days of the Society's psychical research and that of the last twenty years. This difference has resulted from the development of statistical methods as an essential tool in biological research.

The question to be answered is not how likely it is that any excess of right answers is due to psi, but how likely it would be that an excess of at least the size observed would arise if chance alone were operative (i.e., if there were no causal factor tending to make the subject guess right). This is the quantity symbolized in statistical methods by the letter P. If P is sufficiently small (.01 or less), there is adequate ground for concluding that some specific cause must be at work tending to make for deviations from mean chance expectation. It is then a problem, not of mathematics but of experimental design, to ensure that if such a cause is present it must be some paranormal means of guessing correctly.

Applying to the Soal-Goldney experiment with Basil Shackleton (**14**) this criterion of how probable it is that its success would have been found in a chance series, it is found that the odds against it are 10^{35} to 1 ($P = 10^{-35}$). Dr. Soal reports that this astronomical figure of 10^{35} is exceeded in his later series of experiments with Mrs. Stewart (**13**). Even the shorter Pratt-Woodruff series had the enormous odds against occurring by chance of about a hundred billion (10^{14}) to one (**7**).

Figures of this order create an overwhelming conviction that these are not chance results. It has, however, further been objected that these are the selected best results and that if all unsuccessful ESP experiments were taken into account, the total result would not be significantly above chance expectation. This would certainly be a possibility if we had few successful experiments and these showed only small deviations from chance expectation. It is, however, no longer a possibility if we consider the implications of the actual values obtained. We may calculate how many unsuccessful experiments would be needed to reduce to insignificance the Soal-Goldney figure for P of 10^{-35}. A larger number would be required than the whole history of the world allows for. If this were the one

successful psi experiment, it would still remain statistically significant ($P = 10^{-17}$) if one made the fantastic assumption that every inhabitant of the globe had done an unsuccessful parapsychological experiment every month since the beginning of the tertiary period sixty million years ago. 10^{17} to 1 is very heavy odds, far heavier than we should be content to regard as virtual impossibility in everyday life. If we suppose that on the average one person is killed in a century by being struck by a meteor, then 10^{17} to 1 is about the odds against me being killed by a meteor during the next minute.

If we are convinced that the facts reported in experimental parapsychology do really happen, it remains to consider whether they are of any importance. If they were merely amusing oddities, there would be no reason for serious study of them. What makes them interesting is that we should not expect them to happen. They obviously do not fit in with our expectation that behavior will only be determined from outside by sensory disturbances initiated by physical stimuli, and that we can only move objects in the physical world by touching them or starting some train of physical events which terminates in contact with them.

In all science, an unexpected experimental result is a challenge to the basis of our expectations and so becomes a possible starting point of theoretical advance. The failure of the Michelson-Morley experiment to reveal the expected motion of the earth relative to the ether made necessary the reconstruction of the theory on which the expectation of a detectable ether drift was based. By good fortune, this situation also produced the genius of Einstein who was able to take the essential step in the reconstruction of theory which made the Michelson-Morley result explicable.

I suggest that the discovery of the psi phenomena has brought us to a similar point at which we must question basic theories because they lead us to expectations contradicted by experimental results. The history of physics should encourage us to pay attention to the unexpected experimental result and to regard it as an invitation to reconsider the theory which underlies our expectations. The theory must be changed until it leads us to expect that to happen which in fact does happen. In the fact that we have experimental results that are unexpected and inexplicable, we have in parapsy-

chology a situation favorable to a profitable advance in theory.

In other respects, the situation in parapsychology is less favorable for theoretical reconstruction. Anyone familiar with the history of physics during the last half-century must notice how much advance in theory had depended on the results of precise measurement: of the velocity of light, of the energy distribution of different wavelengths in radiation, of deflections of particles moving in electrical or magnetic fields, etc. In the field of parapsychology such precise measurements cannot be made and the testing of alternative hypotheses is, therefore, difficult to perform and somewhat indefinite in its results. The difficulty arises from the fact that in parapsychology the raw data are statistical and not exact measurements, that is, the variance due to uncontrolled causes is of the same order as the variance due to any factor we want to measure. Perhaps it means only that we shall have to accumulate a much larger amount of experimental knowledge before we can see exactly the kind of modification of theory that they require.

I think that one lesson we can learn from the history of theoretical physics is the desirability of flexibility of mind in approaching such problems. The obstacle to new theoretical insights in the past has been rigidity in holding to old conceptions. Because the conceptions belonging to the science of the past may become embedded in the language we use, we are very much inclined to mistake them for necessary truths. It is not, of course, possible now to say which of our old conceptions must be given up in order that we may begin to understand psi. If I knew that, I should be in a position to lay the foundation for the new science which will include psi. I am certainly not in such a position. I can only suggest that we must be ready to question all our old conceptions and to distrust all our habits of thought.

There is one conception in current scientific thought which has a degree of prestige that makes it difficult to question which, nevertheless, I think is perhaps one of the limiting conceptions it may be necessary to discard. This is the system of ideas sometimes called the "psychosomatic theory" which regards all mental phenomena as aspects of physical events in the nervous system. Dr. Wiesner and I have suggested that perhaps the psi process is not essentially paranormal but is the normal process by which I am informed about

external events by the mediation of events in the nervous system, and that paranormal psi processes are simply cases in which this mediation is discarded and I am directly aware of an external event (**19**). Such a view necessarily implies the discarding of the psychosomatic theory since the "I" referred to must be something other than the nervous system itself or any aspect of it. To generalize the word "I" to include other people than myself we have suggested the Hebrew letter "Shin" as symbol. This concept of Shin is obviously related to the old concept of a soul or spirit, although it is better to avoid the use of a term that has previous theological and philosophical implications which we may or may not feel justified in including subsequently in the concept of Shin. Similarly, we suggest that it may be that human volition is a particular example of psychokinesis in which Shin acts on its own nervous system whereas the paranormal psychokinetic phenomena are examples of the same process where the object acted on is external to the body.

We are not prepared to defend this line of speculation very strongly but only to suggest it as one of the kinds of possibility to which it is necessary to keep our minds open. I do not think that any speculations we can make in the present state of knowledge of the subject can be regarded as very important. What is important is that we should maintain flexibility of mind with respect to any possible theoretical developments even if these seem to be a reversion to systems of thought now out of fashion. Above all, the main means by which we can prepare the way for a future Einstein of parapsychology is by accumulating more and better experimental results.

References

1. Bindrim, E. A new displacement effect in ESP. *J. Parapsychol.*, 1947, **11**, 208-21.
2. Carington, W. Experiments in the paranormal cognition of drawings. *Proc. Soc. psych. Res.*, 1940, **46**, 34-151.
3. Fisher, R. A. *The Design of Experiments.* Edinburgh, 1935.
4. Humphrey, B. M. Success in ESP as related to form of response drawings, I. and II. *J. Parapsychol.*, 1946, **10**, 78-106, 181-96.
5. McMahan, E. A. An experiment in pure telepathy. *J. Parapsychol.*, 1946, **10**, 224-42.

6. McMahan, E. A., and Rhine, J. B. A second Zagreb-Durham ESP experiment. *J. Parapsychol.*, 1947, **11,** 244-53.
7. Pratt, J. G., and Woodruff, J. L. Size of stimulus symbols in extrasensory perception. *J. Parapsychol.*, 1939, **3,** 121-58.
8. Rhine, J. B. Experiments bearing on the precognition hypothesis: I. Pre-shuffling card calling. *J. Parapsychol.*, 1938, **2,** 38-54.
9. ———. *Extrasensory Perception.* Boston: Bruce Humphries, 1934.
10. ———. Telepathy and clairvoyance reconsidered. *J. Parapsychol.*, 1945, **9,** 176-93.
11. ———. Terminal salience in ESP performance. *J. Parapsychol.*, 1941, **5,** 183-244.
12. Rhine, L. E., and Rhine, J. B. The psychokinetic effect. I. The first experiment. *J. Parapsychol.*, 1943, **7,** 20-43.
13. Soal, S. G. *The Experimental Situation in Psychical Research,* (Ninth Myers Memorial Lecture). London: Society for Psychical Research, 1947.
14. Soal, S. G., and Goldney, K. M. Experiments in precognitive telepathy. *Proc. Soc. psych. Res.*, 1943, **47,** 21-150.
15. Thouless, R. H. A comparative study of performance in three psi tasks. *J. Parapsychol.*, 1949, **13,** 263-73.
16. ———. Experiments on paranormal guessing. *British J. Psychol.*, 1942, **33,** 15-27.
17. ———. The present position of experimental research into telepathy and related phenomena. *Proc. Soc. psych. Res.*, 1942, **47,** 1-19.
18. ———. Report of an experiment on psychokinesis. *Proc. Soc. psych. Res.*, 1951, **49,** 107-30.
19. Thouless, R. H., and Wiesner, B. P. The psi processes in normal and "paranormal" psychology. *Proc. Soc. psych. Res.*, 1947, **48,** 177-96.
20. Tyrrell, G. N. M. Some experiments in undifferentiated extrasensory perception. *J. Soc. psych. Res.*, 1935, **29,** 52-71.
21. Warcollier, R. *La Telepathie.* Paris: Alcan, 1921.

2 Leys Road
Cambridge, England

MINOR ARTICLES

ON THE SUSCEPTIBILITY OF A MANUALLY OPERATED RANDOM SELECTOR TO PSI DEXTERITY: A PRELIMINARY REPORT

By E. A. G. KNOWLES

ABSTRACT: The experimenter in this series is a teacher of statistics who wished to have her students use a random selector apparatus in connection with their class work. This apparatus consisted of a pointer spun by hand over a paper scale on which was drawn a circle divided into 15 sectors. The scale was hidden by the cover of the apparatus when the pointer came to rest. Since randomness in the operation of the machine is a prime necessity for statistical work the experimenter decided first to determine whether the apparatus could be affected by psychokinesis. She therefore carried out a series of 800 spins with herself and her brother as subjects. The subjects' task was to spin the pointer and then, with the pointer out of sight, to apply a brake. They were to concentrate upon having the pointer come to rest in a specified sector. The subjects scored a statistically significant number of hits in the target sector. It was also found that there was a spread of this effect to the sector on each side of the target sector. The experimenter concludes that these results were due to a psi effect similar to PK rather than to bias in the apparatus and that this finding has practical significance for statisticians who are considering the use of this type of selector.—Ed.

INTRODUCTION

THE IDEA of the experiment described here arose out of the preparation of a course of lectures on statistical theory and experiment in the following way:

It was considered that a manually operated random selector would be a very useful device with which to introduce a practical experiment on statistical phenomena into the lecture course. Such a selector would have to be as simple and effective as possible, and the apparatus to be described here appeared to satisfy the requirements.

Since a very wide and detailed use was to be made of the results of the random selector experiment, it was evident that any factors interfering with randomness would show up in time, if not at once, and would lead to statistically unsatisfactory results. Hence, instead of illustrating statistical theory, the experiment would tend to shake

the students' confidence in the subject. It is therefore a serious mistake, educationally as well as statistically, to neglect the consideration of any factor that could have a bearing on the question of randomness.

Parapsychological research has by now produced much evidence concerning the effect known as psychokinesis and it would seem that this is a factor which one cannot afford to neglect. It can no longer be regarded as adequate from the scientific point of view to use the results of practical statistical experiments with "random selectors" which require human participation without first investigating the susceptibility of the apparatus to parapsychological effects and establishing the precautions which need to be taken in order to make the experiments valid for their intended purpose. Therefore the following series of tests was undertaken to determine whether or not results of a parapsychological nature could be obtained with the apparatus in question.

Description of Apparatus and Method of Operation

The apparatus (Fig. 1) incorporates essentially a pivoted balanced pointer capable of being spun over a printed paper scale (Fig. 3), and a braking device arranged for operation while the spinning pointer is out of sight.

The pointer is spun manually by first gripping the rubber collar between thumb and finger and then applying a rapid twist. The upper board with its brake pad is then immediately brought down by hand to stop the pointer. (Fig. 2)

The scale is a square piece of paper on which is printed a circle the outer edge of which is about seven and one-half inches in diameter. It is divided into 15 equal sectors numbered as indicated in Figure 3. This paper is pierced at the center by means of the pivot which is attached to the pointer, and the pivot, with the paper on it, is inserted into the hole of the brass bush. Thus the apparatus is ready for operation as soon as the scale is rotated into position in such a manner that the edge of the paper nearest to the 0-sector coincides with a predetermined side of the square base of the apparatus. The side of the apparatus nearest to the 0-sector was frequently altered in the present experiment so that evidence could be provided concerning apparatus bias. A new scale sheet was used for each experimental session.

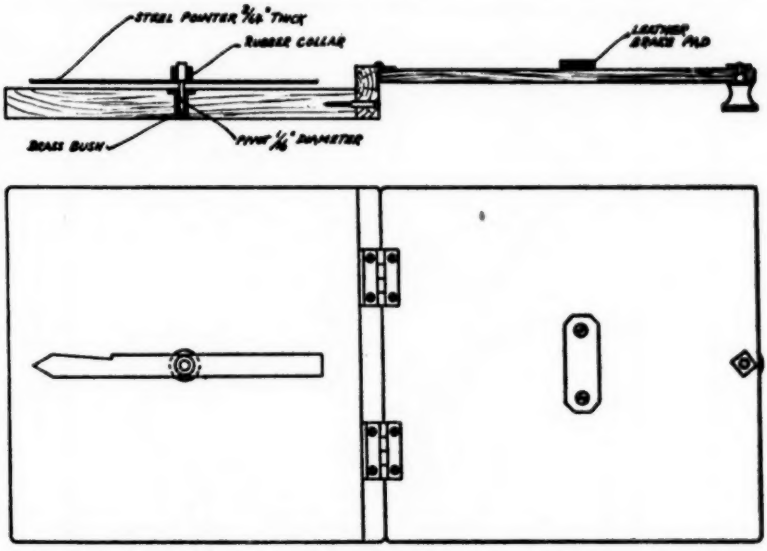

Fig. 1. Side and top views of the apparatus shown without a scale under the pointer.

It will be observed (Fig. 1) that the pointer has one radially directed edge which, if extended, would pass through the center of the scale. This is used as a ruler with which to make a short pencilled line to mark the end position of each spin on the scale. Whenever this line coincides with a boundary line between two scale sectors, the end position is marked at the apex of the pointer, and the trial is counted as a hit in the corresponding sector. In the present experiments this mark is made by the subject (Fig. 2) under the recorder's control. The recorder then makes his entry on a separate record sheet designed to make scores available in the order of their occurrence. Thus two records are kept to be checked against each other. With this precaution it is possible to avoid mistakes and the sort of error that could occur, for example, in the records from the machine for constructing random sampling number tables as described by M. G. Kendall and B. Babington-Smith (**1**, pp. 58-59). The present apparatus might be elaborated to produce automatic recording, thus making increased speed of operation possible.

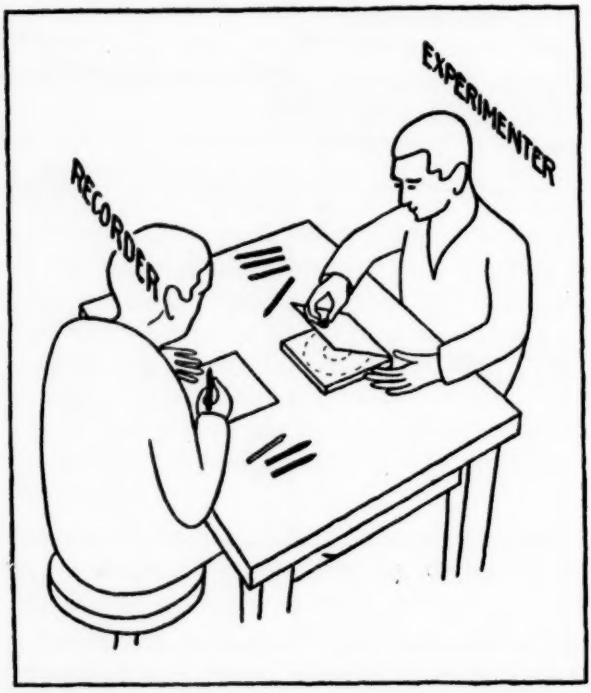

Fig. 2. The experimental situation when two observers were present.

Procedure

One experiment consists of 100 spins of the pointer, stopping in the manner described. Each experiment is divided into 10 runs of 10 spins each. The trials are recorded in vertical columns of 10 entries apiece with a space left in the column between the first five and the last five entries. The recorder and the subject both use four pencils of different colors in marking the records. This is for the purpose of separating the set of 100 trials clearly into four quarters since this division has been found to be of interest in other parapsychological work. Thus, during the first five runs the first five spins of each run are marked with black pencil, the last five spins are marked with red pencil. Pencils are quickly changed in the middle of the run without otherwise interrupting the experiment. Between two runs a short pause of a minute or two is made in order

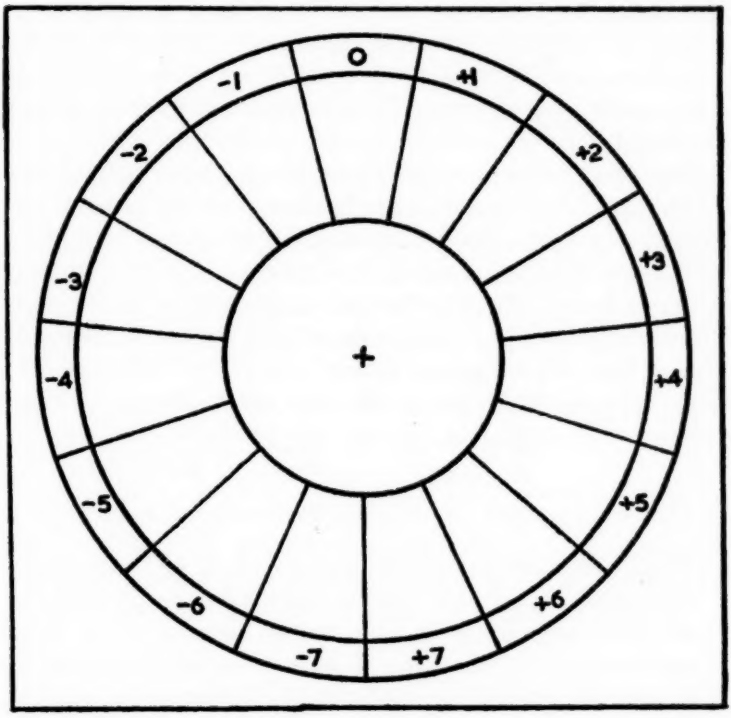

Fig. 3. The paper scale.

to allow the subject to rest. Generally some conversation is carried on during this time. When half the experiment is completed, two new colors are chosen for the records, blue for the first five spins of a run and green for the last five.

The Apparatus as a "Random Selector"

Some of the reasons why this apparatus, simple as it is, was expected to be a good "random selector" are as follows:

a) Bias based on a tendency of the pointer to come to rest in one or more preferred positions due to unequal pivot friction or the like appears to be excluded by the dominant manual force applied to the brake pad at arbitrary instants.

b) Bias due to imperfections of the brake pad with any tendency to arrest the pointer in a preferred position would be neu-

tralized by random alteration of direction of the sheet bearing the scale for successive experiments. (A check on the fulfillment of this point can be made since the direction of the scale with regard to the apparatus was marked on each sheet.)

c) Ability to judge the pointer position visually is ruled out by the pointer's being out of sight for about one or more seconds before the brake is applied. It should be emphasized that this time was variable for the same subject, as is illustrated by the following frequency diagram for a small sample showing how many seconds the pointer was out of sight for each trial (x) the subject made:

1.0	1.1	1.2	1.3	1.4	1.5	1.6	1.7	1.8	1.9	2.0	2.1	2.2
x			x		x		x	x	x	x	x	x
			x		x		x		x			
			x				x		x			
			x				x					
			x									
			x									

These times are so variable because (i) it is known from work-study in industry that the times taken to perform the simplest movement, even for a trained operator in his usual production task, are subject to some variation, even when he is working at what he thinks is his fastest speed; (ii) in addition the effect of wishing or willing tends to introduce further variability, since it frequently causes a little hesitation before applying the brake. In this connection it is important to note that this hesitation tended to occur at the end of the movement (when the pointer was out of sight) immediately before applying the brake force, not immediately after setting the pointer in motion.

d) Since the initial speed of the pointer was found to vary considerably, even for the same subject (200, at least, to 2,000 or more revolutions per minute according to stroboscopic determination) and was not uniform but decelerated at an unknown and variable rate, even extreme accuracy in the initial judgment of the time intervals before the pointer vanished from sight would appear to be of no avail in any attempt to determine the end position of the pointer. The

subjects had no inclination whatever to dwell on the speed of the pointer after setting it in motion. That this could not possibly have helped can be seen from the results of researches on "thresholds for discriminating velocity," given in the *Handbook of Human Engineering Data for Design Engineers* (3) whereby, under more favorable conditions, it is stated that for an angular velocity above 116° per second a well-lit rotating shape appears to the eye as a stationary sheet of light. Even the lowest speed of 200 r.p.m. would exceed this speed by a factor of ten. The difficulty of applying the effort in an identical manner every time accounts for the differences in initial speed.

e) Furthermore, there were differences in the speed of application of the brake pad causing variations in the rate of deceleration.

Hence, bearing in mind all these facts and sources of variation, it seemed justifiable to conclude that under the conditions described the pointer should stop in any one sector with the same probability as in any other; that is, it should select the sectors at random.

The "Random Selector" as an Instrument to Demonstrate Psi Dexterity

If any subjects spin the pointer of the present apparatus and stop it as described, while wishing or willing that it should stop at the sector marked "0," the results should indicate whether or not they are able to achieve a measure of success in their efforts. From the facts given it would appear to be clear to anyone acquainted with the psychology and physiology of manual work that the sensorimotor system of the human body, so far as it has been studied by experimental psychology and physiology to date, could not account for such success. It would seem, therefore, that a statistically significant result indicating a tendency to such success belongs to the phenomena of parapsychology, as defined by J. B. Rhine (2), since it belongs to "those occurrences that seem beyond interpretation by the kind of principles involved in the sensorimotor relation of the individual to his environment." One could not, however, justly claim that a result of this nature is necessarily due to PK, since the manual

effort plays an important part in this experiment and possible psi faculties involved might act through the body rather than directly on the apparatus. It is therefore suggested that the phenomenon of this experiment might be termed "psi dexterity" or PD. Only if PD were excluded could it be claimed that PK alone was operating upon the pointer. (Mechanically thrown dice experiments could be valid as pure PK experiments only if all manual touch by the subject, however slight it might be, were excluded.)

First Questions to Be Answered Tentatively

Apart from the general purpose of the present experiments, which has been referred to, it was hoped that tentative answers to the following particular questions should be indicated by the results:

a) The statistically significant results of experiments with manually thrown dice may be attributed to PK or PD. Are PK or PD effects which tend to occur only with dice, or are they more general effects which can be brought into play on other apparatus, for instance that here described?

b) In the case of throwing a die we are either successful in obtaining a target face, or we are not successful. With the present apparatus we may score hits on "0," but we may also have some "close misses," such as hits on the adjacent sectors +1 and −1. Is PD, if it exists, quite accurate when it operates, or is there evidence that sectors adjacent to the target sectors tend to be affected? Positive answers to these questions can, of course, only be sought in terms of a low probability of the null hypothesis.

Results

Success in Relation to the Target Area on the Scale

In this preliminary report the results of the first complete series of experiments is given. (The series of a subject has been called "complete" if the subject has had four experimental sessions, preferably one for each of the four orientations of the scale.) These data were completed by two subjects, the writer (denoted by the initials E.A.G.K.) and Dr. F. W. Knowles (denoted by F.W.K.). E.A.G.K. acted as recorder for all experimental sessions of F.W.K. and he acted as recorder in the third experimental session of

E.A.G.K. All remaining experimental sessions were recorded by E.A.G.K.

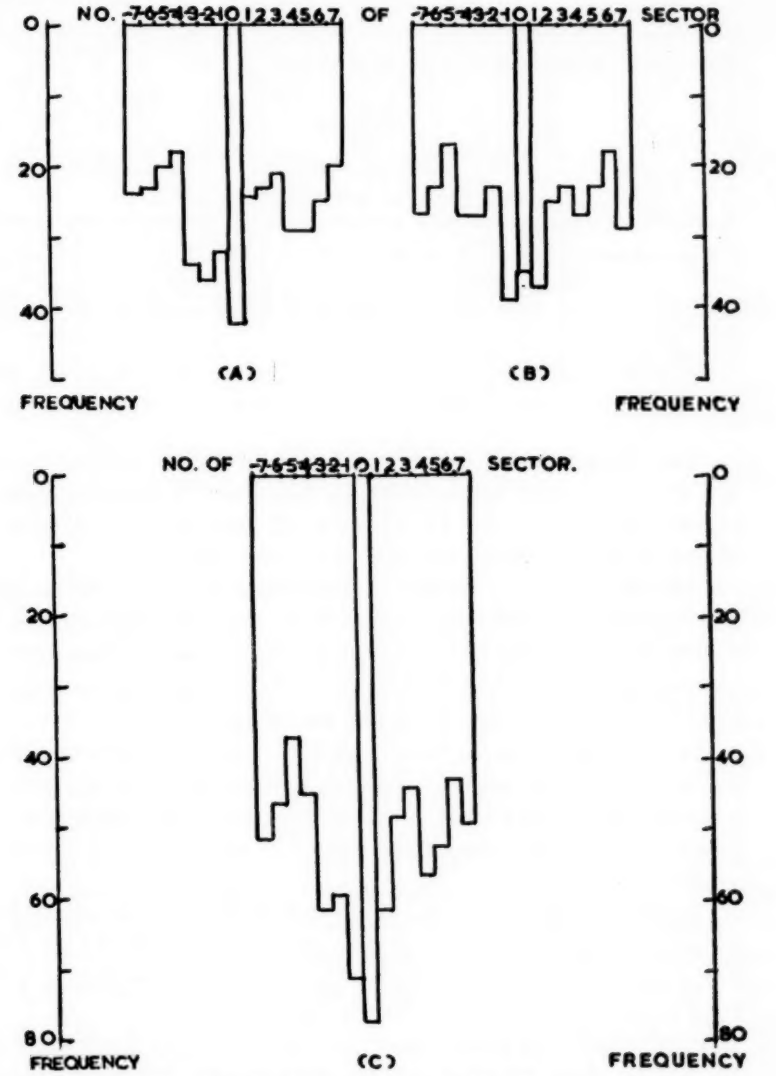

Fig. 4. Distribution of pointer readings for E.A.G.K., represented by (A); for F.W.K., represented by (B); and for both subjects combined, represented by (C).

Figure 4 gives the frequency diagrams of the results: (*A*) represents those of E.A.G.K., (*B*) those of F.W.K., and (*C*) the combined result of the two. There were four experiments for each and therefore the combined results represent the scores for 800 spins. The actual numbers are given in Table 1.

Table 1

GENERAL RESULTS

Pointer Readings in the Fifteen Sectors

Sector of scale	−7	−6	−5	−4	−3	−2	−1	0	1	2	3	4	5	6	7	Totals
Group (*A*).....	24	23	20	18	34	36	32	42	24	23	21	29	29	25	20	400
Group (*B*).....	27	23	17	27	27	23	39	35	37	25	23	27	23	18	29	400
(*A*) and (*B*) Combined.....	51	46	37	45	61	59	71	77	61	48	44	56	52	43	49	800

The average number of hits expected by chance in the 0-sector is 1 in 15. In 800 spins, therefore, 53.33 would represent chance expectation. Hence the 77 hits actually obtained give a positive deviation of 23.67 with a CR of 3.35 (P = 0.0008).

The first tentative answer to question *a* would therefore indicate that PD and PK are general effects and that PD appears to be able to operate on the "random selector" provided the effect is not due to apparatus bias, to skilled operation based on information obtained through the senses, or to a rare chance.

In order to throw light on question *b* it is useful to consider the two groups (*A*) and (*B*) separately as well as the total. Figure 4 shows that in (*A*) there is marked success in the target section, whereas in (*B*) the almost equal success in the −1, 0, +1 sectors is striking.

Table 2 shows that there is a nonrandom distribution of hits characterized chiefly by high scoring in the 0-sector for (*A*). This does not hold for (*B*). However, the sum of the frequencies (*A*) + (*B*) gives a P lower than the one for (*A*).

From Table 3 it is seen that the scores for (*A*) in sectors +1, −1, and 0, added together, become insignificant, whereas the comparable data for (*B*) attain statistical significance. The combined

Table 2

SUCCESS IN THE 0-SECTOR

	−1 or less	0	+1 or more	Totals	χ^2	Degrees of freedom	P
(*A*)	187	42	171	400	10.13	2	.006
(*B*)	183	35	182	400	2.79	2	.25
Chance Expectation	186.67	26.67	186.67				
(*A*) and (*B*) Combined	370	77	353	800	11.64	2	.003
Chance Expectation	373.33	53.33	373.33				

Table 3

SUCCESS IN (0, −1, +1)-SECTORS COMBINED

	−2 or less	−1, 0, +1	+2 or more	Totals	χ^2	Degrees of freedom	P
(*A*)	155	98	147	400	5.26	2	.08
(*B*)	144	111	145	400	15.02	2	.0005
Chance Expectation	160	80	160				
(*A*) and (*B*) Combined	299	209	292	800	18.83	2	.00008
Chance Expectation	320	160	320				

results now attain a high statistical significance (P = .00008). The first tentative answer to question *b* is therefore as follows (with the same proviso as for question *a*): it appears probable that the PD effect, when operating, is not necessarily perfectly accurate, since not only the 0-sector but also the sectors +1 and −1 are affected by an increased number of hits.

The Independence of Success in the Target Area and Orientation of the Scale

It now remains to give some evidence indicating that the success described above is not attributable to apparatus bias. (See the section above entitled, "The Apparatus as a 'Random Selector.'") It will be remembered that the target sector was shifted about among the four sides of the board. Each side of the board thus took in one quadrant of the circle, 3¾ sectors, determined by lines drawn diagonally from the four corners of the board through the center. The marks made by the subject in each quadrant for each

Table 4

DISTRIBUTION OF RESULTS FOR FOUR QUADRANTS OF THE SCALE IN RELATION TO THE FOUR POSITIONS OF THE TARGET ON THE BOARD

Position of Target on Board	NUMBER OF MARKS IN THE QUADRANTS				Totals
	I	II	III	IV	
I	55 (47.00)	57 (55.25)	43 (48.25)	45 (49.50)	200
II	45 (47.00)	72 (55.25)	37 (48.25)	46 (49.50)	200
III	67 (70.50)	71 (82.88)	91 (72.38)	71 (74.25)	300
IV	21 (23.50)	21 (27.63)	22 (24.13)	36 (24.75)	100
Totals	188	221	193	198	800

(The numbers in parentheses are those expected on the assumption of independence between the quadrant marked and the position of the target.)

position were counted and the results are given in Table 4. In making this tabulation the quadrants were numbered I, II, III, IV, with reference to the position of the quadrant on the board irrespective of the position of the target.

Table 4 indicates that the number of marks tends to be greatest in that quarter of the board that contained the target sector; that is, the numbers in the diagonal from top left to bottom right on the table tend to be the greatest. (This diagonal will be referred to as the "main diagonal.")

If there were any significant bias in the apparatus large enough to show up in this sample size, this would tend to produce joint decreases or increases affecting all the numbers of the same column in a similar manner. In consequence, if there were no effect other than chance and bias operating, the four rows should represent samples from the same population. Hence, if the chi-square test applied to the matrix of figures in Table 4 to test this null hypothesis yields a high value of P we would conclude that the larger numbers in the main diagonal are merely chance results and that the number of hits in the target area is not statistically significant.

Table 5

VALUES OF CHI SQUARE FOR RESULTS FOR FOUR QUADRANTS OF THE SCALE IN RELATION TO THE FOUR POSITIONS OF THE TARGET ON THE BOARD

Target in the Quarters	CONTRIBUTIONS TO χ^2 FROM THE QUARTERS:—				Totals
	I	II	III	IV	
I	*1.36*	0.06	0.57	0.41	2.40
II	0.09	*5.08*	2.62	0.25	8.04
III	0.17	1.70	*4.79*	0.14	6.80
IV	0.27	1.59	0.19	*5.11*	7.16
Totals	1.89	8.43	8.17	5.91	24.40

*Therefore $\chi^2 = 24.40$ with 9 degrees of freedom, whence $P = 0.0038$ approximately.

Table 5 shows the χ^2 values for each cell and the total χ^2 derived from Table 4.

From this result we are entitled to doubt the null hypothesis. In fact we see that the largest contributions to χ^2 for each row as well as for each column originate from the terms of the main diagonal, indicating that the excess of scores in the target area are the main cause for the high value of χ^2. (The four diagonal terms contribute 16.34 to χ^2, leaving only 8.06 for the remaining twelve terms.) Hence it appears that apparatus bias is not likely to have caused the successes in the target area. This test therefore fully supports the significance of the first test.

DISCUSSION AND COMPARISON WITH A SKILL EXPERIMENT

These results show that there is some statistical evidence of a tendency by the subject to stop the pointer significantly more often in or near the target area than in other parts of the scale. Many individuals not familiar with the researches of parapsychology would rather conclude that nothing other than a fairly rare chance event has been demonstrated here. This may be so, but it is to be remembered that the effect studied in the results given above has persisted through eight independent experimental sessions, not only by one but by two subjects. It has been explained that the writer's concern in this experiment is an urgent practical interest, and from this practical point of view it matters little what the explanation of the psi factors involved is, or what philosophy may be necessary to

account for them. If procedures such as that described above tend to produce rare chance events, this by itself appears to the writer to have undeniable practical significance.

Comparisons with a Skill Experiment

It may be of interest in this connection to attempt to obtain some sort of measure for the success of the reported series of experiments by comparing it with the results of 400 spins on the "random selector" made by E.A.G.K. in such a way as to give every possible opportunity to the application of sensory perception as well as of skill. Instead of using the brake pad the spinning pointer was kept under unrestricted view by the subject and the brake was applied by means of touch with the index finger, whereby it was permissible to reduce the pointer to rest as slowly as desired. It might be thought that this freedom would make the task so easy that the pointer would be stopped at "0" every time. Surprisingly enough, this is by no means the case. Figure 5 and Table 6 represent the results obtained:

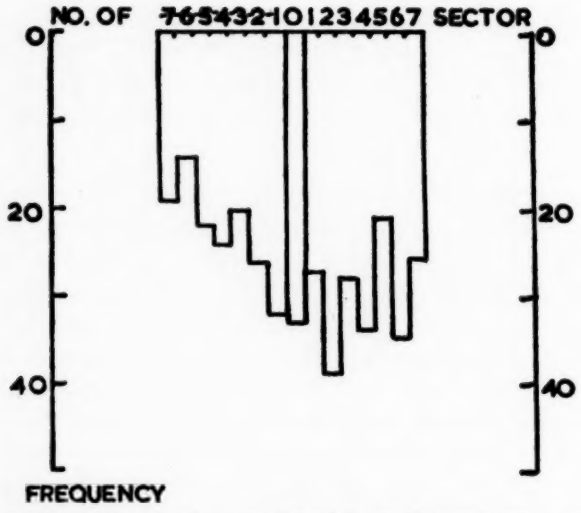

Fig. 5. Distribution of pointer readings in control experiment on motor skill. The experimenter braked the pointer with her index finger while watching it and attempting to stop it on the 0-sector.

Table 6

RESULT OF SKILL EXPERIMENT
Pointer Readings in the Fifteen Sectors

Sector of Scale	−7	−6	−5	−4	−3	−2	−1	0	1	2	3	4	5	6	7	Total
Frequency of Hits..	19	14	22	24	20	26	32	33	27	39	28	34	21	35	26	400

Table 7

SUCCESS IN (0, −1, +1)-SECTORS COMBINED

	−2 or less	−1, 0, +1	+2 or more	Totals	Degrees of Freedom	χ^2	P
Observed.....	125	92	183	400	2	12.76	.0017
Chance Expectation.....	160	80	160				

Application of the same statistical method of test now shows that neither the hits in the 0-sector nor the combined hits in the −1, 0, +1 sectors are significantly high, although it is clear from Table 7 that the departure from chance expectation is significant, as indicated by the value of P = .0017. This time, however, the greatest contribution to χ^2 arises from the deficiency in hits in the sectors −2 or less rather than from an excess in the target area. The excess is more marked in the sectors from +2 to +6. Hence it may be concluded, tentatively, that at least for one subject sensory perception and skill do not always appear to be as effective as PD alone, if the experimental series involves no more than 400 spins. There is, however, a reason for this curious result, which should not be passed over. The pointer was spun by the subject in a clockwise direction, and since it is known that every subject is handicapped by the fact that the motor system of the body cannot act immediately but requires a reaction time, it would seem natural that a large number of possible hits are displaced in a clockwise direction, at least until sufficient time has elapsed for the subject to become practiced as well as to allow for this displacement by aiming at sectors preceding "0" rather than at "0" itself.

Further Questions

It is hoped to obtain answers to the following further questions in time, when additional series of experiments have been completed:

a) Of what nature are the differences between results obtained by different subjects as well as those between the same subjects at different times?

b) On what types of apparatus or machines can PD operate?

c) Has PD a function in the learning process, both for the child and for the adult; for example, a manual worker who changes his work frequently or a sports enthusiast, good at learning new games?

Conclusions

The results of these first experiments appear to indicate the possibility of psi dexterity with respect to the described apparatus. From the point of view of one interested in experimental statistics this is a rather inconvenient finding. However, the writer feels well compensated for these inconveniences by the intrinsic interest of these phenomena, their possible practical importance, and the prospect of further insight into new fields of inquiry.

Personal Notes on the Attitude of the Subjects Toward the Experiment

It seems possible that the success of this experiment is related to a special attitude on the part of the subjects. About twelve years ago F.W.K. conducted some ESP experiments with sealed envelopes. He obtained striking results with several subjects, but not with me. I saw some of those experiments and, being satisfied with their evidence of ESP, I had at that time no further "quest for knowledge." My interest remained a theoretical one until one day a particular practical purpose roused it into action when I organized a dice-throwing experiment under unusual conditions and obtained unexpected significant results.[1] F.W.K. organized many dice experiments; but he obtained no significant results either as a subject or as experimenter.

The PD experiment here reported is, therefore, the first instance

[1] *J. Parapsychology*, 1949, **13**, 186-196.

of successful psi experimentation in collaboration by the two of us. The "random selector" was designed for chance experiments; but the idea arose that psi faculties might affect it, and I was fascinated by the idea of testing this possibility, as it appeared to affect the basis of statistical experimentation. However, if my first hundred tests, which were significant on the three percent level, had not been so striking, I would not have pursued the experiment. A new "quest for knowledge" had arisen, and the reported experiment was performed with enthusiasm which increased to joyful excitement as hits accumulated in the target area.

A short time after my first successful experimental session with myself as subject, F.W.K. saw the apparatus and the first result and became very interested. Apart from the other experimental sessions many discussions took place between us about different aspects of the subject, including the possible nature of the effect. It was he who first noticed that a psi faculty other than PK may be involved—PD. He has also contributed Figures 1 and 2 in this article. All this shows that his interest in the subject was a very active one also.

The difference between a remote theoretical interest and an active interest associated with a live quest for knowledge may be responsible for a difference in intensity of the wish for success. That the intensity of this wish is a deciding factor for success in other mental activities is illustrated in common experience. For example, the wish to read an informative book is not necessarily effective in keeping one's attention on the reading, and little may be taken in unless the wish is reinforced with a live quest for knowledge which heightens the pitch of attention. This heightening of the pitch of attention may be essential for success in psi experiments.

F.W.K. felt that the "random selector" experiment had very great importance for understanding dice phenomena: he had thought for some time that PK was not necessarily operating in any of the significant dice experiments so far published, as they all involved manual operation or some possibility of choice permitting PD. He felt that two factors, PD and PK, may be at work in dice experiments, that experimental demonstration of PD would clarify this and show the need for "pure PK" experiments that exclude PD.

For both of us, then, the experiment here reported was of unusual importance: it was no mere repetition of past work, but involved discovery of new aspects and aroused attitudes of enthusiasm.

References

1. Kendall, M. G., and Babington-Smith, B. Second paper on random sampling numbers. Supplement to *J. Royal Statistical Soc.*, 1939, **6,** 51-61.
2. Rhine, J. B. Parapsychology and physics. *J. Parapsychol.*, 1951, **15,** 81-88.
3. *Handbook of Human Engineering Data for Design Engineers.* Technical Report No. SDC 199-1-1. Dept. of the Navy, Office of Naval Research. NAV EXOS P-643.

107 Harborne Road
Birmingham 15, England

A FURTHER STUDY OF THE PK PLACEMENT EFFECT[1]

By H. Forwald

ABSTRACT: This article reports Section 2 of a PK placement experiment with dice. Whereas Section 1, previously reported, involved the throwing of ten wooden dice at a time, Section 2 involved the throwing of combinations of six dice made of different materials: wood, paper, bakelite, aluminum, and steel. The purpose of Section 2 was to get information about factors favoring success, particularly the effect of differences in the material of which the dice were made. The 27,000 die-throws of Section 2 gave scores on the chance level, but when combined with the high scores of Section 1, they gave a total score for both sections which is still highly significant. The experimenter attributes the lower level of scoring in Section 2 to a possible difference in motivation. Differences in the material of the dice produced only chance variations. Scores for the two sections combined show a U-curve in the five-throw unit of the test which is not attributable to chance.—Ed.

Most of the PK research has involved the attempt to cause specified faces of objects such as dice to turn uppermost after throwing. Placement PK, on the other hand, is concerned with causing dice, or other objects, to come to rest in a designated area of the dice table.

Four reports on placement PK describing the investigations of Cox (1), Cormack (2), L. E. Rhine (4), and the first stage of my own researches (3) have already been issued. In the last-mentioned paper (written by J. B. Rhine) my first two long series of throws were reported and discussed. In the present paper further consideration of that work is given, together with an account of nine additional experimental series.

The apparatus for all the series consisted of an electrically controlled dice container perched at the top of a sloping runway which curved downward and lengthened horizontally into a walled-in dice table. When I, the subject, pressed an electric button at the end of a cord I released a row of dice which rolled down the incline and scattered over the table. This table was divided longitudinally into

[1] The author acknowledges with thanks the assistance of J. Fraser Nicol of the Parapsychology Laboratory in the analysis of the data of this report and in the preparation of the manuscript.

two equal areas, designated as *A* and *B*. During the first series of the previously reported research, the areas were separated by an ink line. During Series 2 of that research, and in all the later work, the division was made by a wire stretched across the surface down the length of the table. It was of sufficient thickness to give a noticeable incline to any cube resting upon it. During the testing I was both the subject and the recorder at the same time.

In the early section of the investigation ten cubes were thrown at a time, all made of the same substance, wood. Five throws of the ten cubes were made for target *A*, followed by five throws for target *B*. This order constituted a "set," and in the last 10,000 trials of Series 2 this order was always alternated with the converse order 5B, 5A. The two experimental series comprised 15,000 and 25,000 throws. There were also two control series (A and B) of 1,000 and 10,000 throws respectively. In the experimental series the number of hits in excess of expectation was highly significant (CR = 5.00); and in the control series the number of hits was somewhat below expectation (CR = 1.74).

Several novel experimental conditions were introduced during the nine series making up the new work (hereafter called Section 2 of the experiment):

1. Six cubes were thrown at a time instead of ten.
2. The cubes were made of five different substances—wood, paper (hollow), bakelite, aluminum, and steel, with a view to detecting whether the placement effect varied with the nature of the substance or its mass. The cubes in some series were all of the same material; in others there were three of one material and three of another; and in one series there were two cubes of each of three different materials.
3. Each series was limited to 3,000 throws.

General Results

The main purpose of Section 2 was to inquire a little more closely into some of the factors favoring success in placement PK and in particular to determine whether the different materials of which the cubes were made would be associated with different levels of scoring. It was also intended, if possible, to relate scoring levels

to the gross masses of the objects thrown. In terms of the actual results of the experiments, most of the above and some associated questions proved unanswerable. Certain of them will be the subject of later research.

From Table 1 it will be seen that the results of Section 2 present a strong contrast to the previously published work of Section 1. In the latter, the scores were significant with a probability of about 1 in two million; in the new work of 27,000 throws, the total deviation is only +57 and the probability is about 1 in 2. Moreover, an inspection of the separate series in Section 2 shows nothing of significance. The highest critical ratio is 1.83, and this is only for one subseries of a thousand throws.

When both sections are pooled, the deviation for the entire 67,000 throws is +557 with a CR of 4.30, which is highly significant (P = .000,017).

Some of the cube materials were used in more than one series in Section 2. When the results obtained with each material are pooled, we get the following deviations:

	Paper	Wood	Aluminum	Bakelite	Steel	Totals
Throws	4,500	5,500	8,500	7,000	1,500	27,000
Deviation	+35	−15	+54	−21	+4	+57

These deviations are negligible also, and they point to one conclusion: the reaffirmation of the null hypothesis.

One result, however, is of slightly greater interest. In Series 8 three kinds of cubes were used: two wood, two bakelite, and two aluminum, all thrown of course at the same time. The expected score in each case was 500; the actual scores were:

Aluminum	Bakelite	Wood
525	493	471

The chi square for the differences between these scores is 5.90. With two degrees of freedom P = .05. If this result is taken as suggestive, then it might be cautiously surmised that PK operated positively on the aluminum cubes by discharging them into the target area, and negatively on the wood cubes by causing them to settle in the non-target area. On the bakelite cubes, the result was to disperse the cubes almost equally into both areas.

It is difficult to explain the apparently sudden deterioration in the placement PK of this new Section 2 as compared with the old

Table 1

TOTAL PLACEMENT RESULTS

Series	Material	Die Throws	Deviation A-Side	Deviation B-Side	Total Deviation	CR	P
			SECTION 1				
1.....	Wood	15,000	+223	− 76	+147	2.40	.016
2.....	Wood	25,000	+504	−151	+353	4.46	.000,009
Total..		40,000	+727	−227	+500	5.00	.000,000,6
Control A.	Wood	1,000	+ 2	− 15	− 13	.82	.41
Control B.	Wood	10,000	+103*	−181	− 78	1.56	.12
Control Total..	.....	11,000	+105	−196	− 91	1.74	.08
			SECTION 2				
3.....	Paper	3,000	+ 29	− 19	+ 10	.37	.72
4.....	Wood	3,000	+ 33	− 11	+ 22	.80	.42
5.....	Paper	1,500	+ 7	+18	+25	1.29	.20
5.....	Wood	1,500	+ 8	−16	− 8	.41	.68
	Total	3,000	+ 15	+ 2	+ 17	.62	.53
6.....	Aluminum	3,000	+ 21	− 36	− 15	.55	.58
7.....	Bakelite	3,000	+ 17	− 7	+ 10	.37	.72
8.....	Wood	1,000	0	−29	−29	1.83	.07
8.....	Bakelite	1,000	+ 2	− 9	− 7	.44	.66
8.....	Aluminum	1,000	+29	− 4	+25	1.58	.11
	Total	3,000	+ 31	− 42	− 11	.40	.69
9.....	Aluminum	3,000	+ 74	− 26	+ 48	1.75	.08
10.....	Bakelite	3,000	+ 51	− 75	− 24	.88	.38
11.....	Aluminum	1,500	+20	−24	− 4	.21	.84
11.....	Steel	1,500	+29	−25	+ 4	.21	.84
	Total	3,000	+ 49	− 49	0	.00	....
Total..	.....	27,000	320	−263	+ 57	.69	.49
			BOTH SECTIONS POOLED (Excluding Controls)				
Grand Total..	.....	67,000	+1,047	−490	+557	4.30	.000,017

*In the previously published report (3) of Section 1, there was a one-point error in the deviation for A-side in Control Series B (and consequently in the totals for the control series).

Section 1. Undoubtedly the explanation lies in some important, though subtle, change in my motivation. The difference that suggests itself as the one responsible is the fact that in Section 2, my main objective was not necessarily to repeat the high scoring of the initial series, though this would have been very desirable, but to determine the situations in which the PK placement effect was mani-

fest and the circumstances in which it varied. This was a more analytical interest than that which was predominant during Section 1, when I primarily wished to see if I could get significant placement results.

Position Effects

An investigation of the position effects of Sections 1 and 2 has given interesting results. It will be remembered that a "set" consisted of five throws of the cubes for each of the two target areas. The scoring rates for each of the five throws (*A* and *B* sides pooled) were computed for comparative purposes and are shown in Table 2 for each of the placement series and the controls. It will be seen that in Section 1 there is a general tendency toward high scoring

Table 2

Position Effects

Series Number	Cube Materials*	Throw Number 1	2	3	4	5	χ^2 (Quadratic Curve)	P
		Section 1						
1	W	+ 31	+33	− 2	+38	+ 47		
2	W	+128	+12	+50	+37	+126		
Total		+159	+45	+48	+75	+173	7.17	.007
		Section 2						
3	P	+ 6	−12	− 5	+17	+ 4		
4	W	− 7	+16	+15	− 8	+ 6		
5	P+W	− 1	+23	0	− 4	− 1		
6	A	+ 17	−15	−11	+ 1	− 7		
7	B	+ 15	− 5	−11	− 2	+ 13		
8	W+B+A	+ 17	−13	−25	+ 8	+ 2		
9	A	+ 16	+17	−21	+24	+ 12		
10	B	− 1	+10	+ 2	−26	− 9		
11	A+S	+ 8	+ 4	0	−12	0		
Total		+ 70	+25	−56	− 2	+ 20	2.58	.11
Grand Total		+229	+70	− 8	+73	+193	10.96	.0009
		Control Series, Section 1						
A	W	− 14	+ 3	− 5	− 3	+ 6		
B	W	− 41	+ 3	+11	−46	− 5		
Total		− 55	+ 6	+ 6	−49	+ 1	0.77	.38

*A = aluminum, B = bakelite, P = paper, S = steel, W = wood.

at the first throw, a decline at throws two and three, and a striking recovery at four and five. The form of the polynomial curve which most closely represents such data is the "quadratic" or U-shaped curve, that is, a drop in scoring level followed by a rise.[2] When such a curve is fitted to the data, the corresponding chi square is found to be 7.17 with one degree of freedom. It has a probability of .007. Thus the decline of scoring level followed by recovery cannot reasonably be attributed to chance variation.

In Section 2 the nine series show little evidence of positional regularity when compared with each other, but in combination the totals for the five places do show the same general trend of fall-and-rise as in Section 1. The effect, however, is too small to be significant ($\chi^2 = 2.58$; P = .11). Nevertheless, when the two sections are taken jointly, the position effects are intensified, giving a chi square of 10.96 and a probability of .0009.

On the other hand, the position effects in the 11,000 control throws bear no resemblance to those of the experimental series. As Table 2 shows, the chi square for the quadratic curve is small (0.77), and the probability is insignificant (P = .38). Evidently the psychological conditions of the two situations—consciously trying to influence the cubes *versus* consciously trying not to do so—produce marked differences in the scoring rates in the five positions.

Discussion

Taken in its entirety, the investigation has given significant results both in terms of total deviation and in terms of position effects. The significance is centered primarily however in Section 1

[2] The method of testing the significance of linear regression, parabolic or quadratic regression fitted to deviations from linear, etc., is standard statistical methodology. A reference for this is "Statistical Methods" by George Snedecor, The Iowa State College Press, 4th edition, 1946, pp. 409 ff.

In the present illustration let X_i stand for the sum of the n scores obtained at throw number i, $i = 1, 2, \ldots, 5$. Let σ be the standard deviation of a single score. Then

$$\chi^2 = \frac{(2X_1 - X_2 - 2X_3 - X_4 + 2X_5)^2}{14n\sigma^2}$$

with one degree of freedom. $\sigma^2\chi^2$ is the amount by which the total sum of squares among the score means of the five throw numbers is diminished when the deviations from a least squares line through the five score averages are measured from a least squares parabola. If this amount is significantly large then it may be asserted that the data tend significantly to follow a parabola. In any event the successive reductions due to the linear, quadratic, cubic and quartic regressions found similarly to the above will exactly account for the total sum of squares among the five score means.

where ten cubes were thrown at a time. None of the series with six cubes per throw gave significant deviations or significant position effects. A number of differences between the psychological and physical conditions of the two sections of the experiment might be cited, but to no one of them nor to any combination of them can the differences in the scoring rates of the two sections be confidently attributed at present.

The U-curve of success found in the five-throw test unit is a familiar phenomenon in psi experiments. It showed up strongly in Section 1, and although the totals of Section 2 show the U-shape, only a few of the individual series reflect that pattern. Again it is impossible without additional experimentation to explain the difference in psi effects in the two sections of the research.

In the course of the experiments, however, another type of position effect appeared; and then, as is frequent in parapsychology, once the phenomenon was noticed, it was found in one of the later series. The effect was strong enough in the early series to warrant its being put on record in the event that it may be encountered again or be of some use in planning other research. In the whole of Section 2 the order in which the six cubes were thrown was: five times for *A*-side of the target area, five times for *B*-side; then five times for *B* again, followed by five times for *A*. Thus there was a contrasted ordering of the targets; namely, the *AB* order and the *BA* order. Now at the end of Series 4 it occurred to me to compare the scores for the two orders and I was surprised to find substantially higher scoring for the *AB* order than for the *BA*. On looking back to Series 3, I found the same phenomenon, and it was also discovered in the last 10,000 trials of Section 1 in which the *AB*, *BA* order was consistently used. When these portions of the data were pooled (that is, Series 3 and 4 and the 10,000 trials of Section 1), it was found that the *AB* order was quite positive (CR = 4.56), while the *BA* order gave only a negligible positive deviation. The CR of the difference between the two orders was 2.56. At this point there seemed to be little doubt about the reality of the effect.[8]

[8] It is interesting to observe that 10,000 die-falls which were recorded during the period covered by this analysis without any conscious intention on my part to influence the results (Control Series B) also gave a higher score on the *AB* order. In this control series, the 5,000 die-falls made in the *AB* order gave a deviation of —10 from chance expectation, while those made in the *BA* order gave —68. Since the PK process is one of which the subject is not conscious, it seems reason-

The sequel is not less surprising. In all succeeding series, 5 to 11, a watch was kept in the expectation that the order effect would continue to repeat itself, but it never did so. It vanished at the hour of its discovery. I am unable to explain either the cause of the original effect or its sudden disappearance. The CR (2.56) may not be regarded as independently significant, of course, since it involves an incomplete selection, and since it is based upon data which were already familiar to me before the analysis was made. The effect has not, therefore, been put to a crucial test. However, it is interesting as supporting the suggestion that a genuine effect may have been occurring which disappeared upon the recognition of the effect by the subject.[4]

Of the PK placement work considered as a whole, my impression is that the cubes are difficult to affect when first released from the trap or during their fall on the incline and that they are influenced mainly on the last part of their motion when velocity is reduced. This view is in keeping with that expressed by Pratt on the Cormack PK placement experiments (2). A certain spread of the cubes is possibly necessary to allow for means of a differential kind to come into action upon the individual cubes. It is conceivable also that in the early part of the run those cubes which have already entered the target area are never acted upon at all, the PK influence being confined to the ones which are moving inside the non-target field.

Some considerations respecting the psychological conditions which may have affected success or failure may be of interest. My impression is that as a rule I have scored positively when I could fully and confidently concentrate on the target, but then when I have been in the slightest way anxious lest the cubes would make their way to the non-target side, I have regularly missed the target.

able to suppose that my intention not to influence the dice may simply have misdirected PK to produce the negative deviation. If so, the order effect found in the experimental series immediately preceding and following this series may have continued without interruption during the control releases. This possibility is mentioned, not with the object of strengthening the evidence for the order effect, but as an observation that may give a clue regarding the nature of PK.

[4] In two series done more recently than those covered in this report, Numbers 12 and 13, there are indications that the order effect has reappeared at a suggestive level (CR = 2.01, P = .044). These two series were done after I had ceased to expect this effect and they were carried through to the end before the data were examined for the *AB* — *BA* decline, both of which facts may have favored the reoccurrence of this special effect.

This circumstance would indicate that consciousness often plays a part in producing the PK effect. I do not consider that the whole thing is to be regarded as an entirely unconscious process.

As a rule I concentrated on the target for five to ten seconds before releasing the cubes, my intention being to await a pre-feeling of success. The button was pushed at the moment when I felt that "contact" with the cubes had been established. Sometimes the "contact" feeling could not be obtained, yet there were many occasions when it was very strong, and my experience was that this mostly happened when my place relative to the mechanism was so as to "pull" the cubes towards me. In such cases I regularly stood (on the concrete floor) somewhat to one side near the end of the table and at a distance of about one meter.

Thouless' experiences (**5**) concerning the psychological conditions in PK work were very much like my own. Also his suggestion that total results should not be assessed until the experiment is completed is, in my opinion, sound advice if good results are to be obtained.

References

1. Cox, W. E. The effect of PK on the placement of falling objects. *J. Parapsychol.*, 1951, **15,** 40-48.
2. Pratt, J. G. The Cormack placement PK experiments. *J. Parapsychol.*, 1951, **15,** 57-73.
3. Rhine, J. B. The Forwald experiments with placement PK. *J. Parapsychol.*, 1951, **15,** 49-56.
4. Rhine, L. E. Placement PK tests with three types of objects. *J. Parapsychol.*, 1951, **15,** 132-38.
5. Thouless, R. H. A report on an experiment in psychokinesis with dice and a discussion on psychological factors favoring success. *J. Parapsychol.*, 1951, **15,** 89-102.

Box 4611
Ludvika, Sweden

BOOK REVIEWS

DET OCKULTA PROBLEMET (The Psychic Problem). By John Björkhem. Uppsala: J. A. Lindblad's Förlag, 1951. Pp. 194.

The book *Det Ockulta Problemet* was first published in 1939; this is an expanded and revised edition. The author, a practicing psychiatrist with graduate degrees in medicine, philosophy, and theology, has published two other books mainly concerned with hypnosis.

The present work is first of all an introduction to parapsychology. The various subfields, clairvoyance, precognition, etc., are treated historically and illustrated with spontaneous and controlled cases from the literature and from Björkhem's own observations. The problems of the field are well discussed: the internal difficulties with the elusive subject matter, and the external difficulties with an outside hostile world. But for persons already acquainted with parapsychology in a general way the main value of the book lies in the new material which it brings to light.

Dr. Björkhem has carried out no less than thirty thousand tests of ESP on hypnotized subjects in Uppsala and Lund between the years 1930 and 1950, using three thousand students as subjects. He is therefore probably the first to make such an extensive use of hypnosis in large scale psi research. Only a few of his experiments, however, have been published in this book.

The author is convinced that hypnosis may produce success in subjects who, in the waking state, exhibit no psi faculty. It is when he makes use of this procedure in psychometry experiments[1] that he has the most interesting results. Contrary to Professor Rhine and others, he does not think that psychometry is a species of clairvoyance or telepathy. The psychometry subject, he says, does not "see" the color of cards in closed envelopes nor can he read sealed letters. But he is able to tell how the letter writer looks and what his habits and circumstances are. A good psychometry subject often participates in the events which he relates. At times all the senses may

[1] Token-object tests of ESP in which the subject's responses are in the form of statements intended to be relevant to events and persons associated with the token-object.

be at work. If a desert is associated with the object he will be blinded by the sun, feel thirsty, etc.

Dr. Björkhem suggests that psychometry may be a more useful concept than telepathy or clairvoyance for explaining some of the other phenomena of the field. Haunted houses are thus "objects" which have such strong emotions attached to them that they create effects perceptible to anyone. In mediumistic séances it is a capacity of this nature which the medium utilizes. In such cases those present play the role of the objects of the psychometry experiment. Björkhem goes so far as to maintain that what it takes a medium years to learn can be achieved by a hypnotized psychometry subject in less than ten minutes: his arguments in favor of psychometry as an experimental procedure and as a concept to cover other psi phenomena are very strong. What still remains to be done is to relate it to the other modes of ESP, such as clairvoyance and precognition. Here it seems that one would carry his thought to its conclusion by suggesting "object reading" as the prototype also of these other forms; for in all ESP there is, if not a material object held by the subject, at least a thought entertained by him and related to the ESP stimulus. Whately Carington called this a "K-idea."[2] Unfortunately his system did not allow for clairvoyance. We might instead turn to field theory; "the psychological approach which regards the barrier between individual and environment as indefinite and unstable, and requires the consideration of an organism-environment field whose properties are studied as field properties."[3]

What may then appear and be experienced as remote from the individual in space and time may therefore, nevertheless, be a part of the field system which constitutes his personality. Björkhem observes that the object used in psychometry experiments is not only a means for concentration but the center around which the events recalled occur. By means of it the stimulus, formerly unrelated to the subject, may become one of his field properties and be known and reported. (In other ESP tests the same purpose may be served by means of thoughts, for example, having to do with the spatial and temporal location of the cards.)

[2] Whately Carington, *Thought Transference.* New York: Creative Age Press, 1946. See p. 103 ff.

[3] Gardner Murphy, *Personality.* New York: Harper and Brothers, 1947. See p. 986.

Dr. Björkhem does not use a statistical appraisal of his psychometry results as, for example, Dr. Hettinger did. But he does suggest that after an object has been obtained from an outsider (to preclude telepathy from the experimenters) it should be submitted to a control group consisting of some three or four experts in various fields. Each of these should write down where he thinks the object comes from, what its origin is, etc., and these statements should later be compared with those the subject has made.

The object is always hidden from the sight of the subject but the latter is allowed to touch the envelope or package containing it. The light should be subdued during the test and the subject should be put into a hypnotic trance.

In some of his experiments Björkhem uses several objects with different associations because he has found that one thing may evoke a response in one but not in another subject. He also observes that if two subjects respond to the same object they will nevertheless select different aspects of its history. (But one object will always affect the same person in the same way.) Such findings fit well into what we know about perception and memory. In all such functions we select and reject. But that which is selected and rejected may throw light upon our personality structure. Do the events selected by the same psychometry subject tend to have similar emotional connotations? This might be one place to look for correlating personality traits.

Dr. Björkhem finds that time order plays no role in determining the sequence of responses in psychometry tests. Those aspects of a thing's history to which the strongest emotions are associated will tend to become manifest first. Even imaginary events may come forth if only the associated emotions are strong. One of his experiments was considered a failure until it was discovered that a wish and not an actual state of affairs had been conveyed. Such findings emphasize the fact that the human personality is an interrelated whole and that none of its functions successfully can be studied in isolation.

Some of Dr. Björkhem's findings resemble the spontaneous case insofar as the results were not anticipated at the outset of the experiment. During one clairvoyance experiment the hypnotized sub-

ject was asked to describe some distant place. This he did correctly. At the time of the experiment the persons at that location, although ignorant of the proceedings, clearly felt that the subject, whom they knew, was present.

It is not the author's intention in this book to provide a full description of his work. But the material presented here certainly does stimulate interest in his experiments. One should like to learn more about his over-all results and have more detail about the hypnotic procedure. Research of this nature holds much promise. It can be tied up with present psi-personality studies. For example, traits favorable to psi may be experimentally evoked by hypnotic suggestion.

W. G. ROLL.

Mr. Roll is a graduate student in philosophy and parapsychology at Oxford University.

VORDERINGEN EN PROBLEMEN VAN DE PARAPSYCHOLOGIE (Progress and Problems of Parapsychology). By F. A. Heyn and J. J. Mulckhuyse. Delft: Uitgeverij. Waltman, 1950. Pp. 101 + 2.

This small book was composed on the occasion of the thirtieth anniversary of the founding of the Dutch Society for Psychical Research, and it is the authors' purpose to give the reader an impression of parapsychology as it developed during those thirty years. They do not profess, however, to offer a complete survey of the field. Rather, this is the type of book which is useful for beginning students who want to get an over-all idea of the subject.

In brief, the book deals with the following material: the first chapter outlines the various types of psi phenomena—extrasensory perception, psychokinesis, and post-mortem manifestations—and describes the methods of collecting and studying data. The latter are defined as the S-method (or the spontaneous case method) and the E-method (the experimental method). Ensuing chapters describe each type of psi phenomenon in detail; two chapters deal with the beginnings of a "theory" and a historical summary of the field; and the final chapter consists of a list of literature subdivided according to subject matter, with remarks about the quality and value of various of the publications.

The authors, as scientists, have tried to maintain an objective attitude; hence they are critical with respect to their choice of material and careful in their conclusions, and they avoid speculation and metaphysical discussion. Even among parapsychologists themselves there are widely divergent opinions on certain aspects of the field. On these questions the book indicates as far as possible what is unanimously adopted and what is not. Only after it has been found that sufficient evidence for the existence of the phenomena is available do the authors give more observation and further detail.

Telepathy is accepted by the authors as a proven fact. They consider the material for clairvoyance and psychokinesis insufficiently conclusive, though, and that on materializations and post-mortem manifestations entirely inadequate for any conclusions, one way or the other.

The authors use a new set of parapsychological terms to describe psi phenomena. To suggest "paragnosis" and "parergis," for instance, instead of "extrasensory perception" and "psychokinesis" may be logically justified, but these changes are not likely to supplant the old terminology and may therefore be the source of some confusion to new students of the field.

HENDRIK RUBINGH.

Mr. Rubingh, a Dutchman himself, is research assistant at McDonald Observatory, Fort Davis, Texas.

GLOSSARY

In order to avoid constant redefining of commonly recurring terms in papers appearing in this JOURNAL, the following definitions are submitted for convenient reference.

A simple description of the basic experimental methods and of the statistical procedures to be followed in evaluating ESP (both card tests and drawings tests) and PK may be found in the *Handbook of Tests in Parapsychology* by Betty M. Humphrey (published by the Parapsychology Laboratory, Duke University, Durham, N. C.).

AGENT: In tests for telepathy, the person whose mental states are to be apprehended by the percipient. In GESP tests, the person who looks at the stimulus object.

AVERAGE SCORE: Average number of hits per run.

BT ("Broken Technique"): The clairvoyance technique in which each card is laid aside by the experimenter as it is called by the subject. The check-up is made at the end of the run.

CALL: The ESP symbol selected by the subject in trying to identify a target.

CHANCE: The complex of undefined causal factors irrelevant to the purpose at hand.

CHANCE EXPECTATION = MEAN CHANCE EXPECTATION: The most likely score if only chance obtains.

CHANCE AVERAGE: Mean chance expectation expressed as an expected score, generally in terms of average per run.

CHI SQUARE: A sum of quantities each of which is a deviation squared divided by an expected value. Also a sum of the squares of CR's.

CLAIRVOYANCE: Extrasensory perception of objective events as distinguished from telepathic perception of the mental state of another person.

CR (CRITICAL RATIO): A measure to determine whether or not the observed deviation is significantly greater than the expected random fluctuation about the average. The CR is obtained by dividing the observed deviation by the standard deviation. (The probability of a given CR may be obtained by consulting tables of the probability integral, such as Pearson's.)

CR OF THE DIFFERENCE: The observed difference between the average scores of two samples of data divided by the standard deviation of the difference.

DECK: Twenty-five ESP cards.

CLOSED DECK: An ESP deck composed of five each of the five symbols.

OPEN DECK: An ESP deck made up of the ESP symbols selected in random order.

DEVIATION: The amount an observed number of hits or an average score varies from the mean chance expectation or chance average. A deviation may be total (for a series of runs) or average (per run).

DIE THROW: The elementary unit of data in a dice-throwing test represented by the throwing and reading of a single die, no matter how many dice are thrown at a time.

DISPLACEMENT: ESP responses to targets other than those for which the calls were intended.

BACKWARD DISPLACEMENT: ESP responses to targets preceding the assigned targets (the ones for which they were intended). Displacement to the targets

one, two, three, etc. places preceding the assigned target are designated as (−1), (−2), (−3), etc.

FORWARD DISPLACEMENT: ESP responses to targets coming later than the assigned targets. Displacement to the targets one, two, three, etc. places after the assigned target are designated as (+1), (+2), (+3), etc.

DT (Down Through): The clairvoyance technique in which the cards are called down through the deck before any are removed or checked.

EMPIRICAL CONTROL: An experiment which wholly or partially follows the main experiment with the exception that the conditions are designed to exclude the possibilty of the hypothesis being tested.

ESP (EXTRASENSORY PERCEPTION): Response to an external event not presented to any known sense.

ESP CARDS: Cards, each bearing one of the following five symbols: star, circle, three parallel wavy lines (called "waves"), square, plus.

ESP SYMBOLS: See plate opposite page 1, this JOURNAL, Vol. 1, March, 1937.

EXPECTATION; see CHANCE.

EXTRACHANCE: Not due to chance alone.

FREE MATERIAL: The stimulus objects of experiments in which an unlimited or unspecified range of stimulus objects is employed (as contrasted with methods such as card-calling in which the subject knows that the stimulus object is one of a known range).

FREE RESPONSE METHOD: An ESP test method in which a relatively unlimited range of stimulus objects is used and in which the subject is instructed to express freely as his response any range of thoughts and ideas.

GESP (GENERAL EXTRASENSORY PERCEPTION): A technique designed to test the occurrence of extrasensory perception, permitting either telepathy or clairvoyance or both to operate.

HIGH-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling eight or more.

LOW-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling six or less.

MEAN CHANCE EXPECTATION; see CHANCE.

P (PROBABILITY): A mathematical estimate of the expected relative frequency of a given event if chance alone were operative.

PARAPSYCHOLOGY: A division of psychology dealing with those psychical effects which appear not to fall within the scope of what is at present recognized law.

PERCIPIENT: The person who makes the calls in an ESP test.

PK (PSYCHOKINESIS): The direct influence exerted on a physical system by a subject without any known intermediate physical energy or instrumentation.

PRECOGNITION: Cognition of a future event which could not be known through rational inference.

PREFERENTIAL MATCHING: A method of scoring free responses. A judge ranks the stimulus objects (usually in sets of four) with respect to their similarity to, or association with, each response; and/or he ranks the responses with respect to their similarity to, or association with, each stimulus object.

PSI: A general term to identify personal factors or processes in nature which transcend accepted laws. It approximates the popular use of the word "psychic" and the technical one, "parapsychical."

PSI PHENOMENA: Occurrences which result from the operation of psi. They include the phenomena of both ESP (including precognition) and PK.

QD (QUARTER DISTRIBUTION): The distribution of hits in the record page (or in a logical subdivision thereof, such as the set or the half-set) as found in the four equal quarters formed by dividing the selected unit horizontally and vertically.

RUN: A group of trials, usually the successive calling of a deck of 25 ESP cards or symbols. In PK tests, 24 single die throws regardless of the number of dice thrown at the same time.

SCORE: The number of hits made in one run.

TOTAL SCORE: Total of scores made in a given number of runs.

AVERAGE SCORE: Total score divided by number of runs.

SD (STANDARD DEVIATION): Usually the theoretical root mean square of the deviations. It is obtained from the formula $\sqrt{npq}$, in which n is the number of single trials, p the probability of success per trial, and q the probability of failure. (For ESP cards, SD $= 2\sqrt{\text{no. of runs.}}$) Sometimes the SD used is that estimated from the observed variability in the scores.

SD OF THE DIFFERENCE: For both ESP cards and PK tests using dice, the SD of the difference is equal to $\sigma_s \sqrt{1/R_1 + 1/R_2}$ where σ_s is the SD of a single run and R_1 and R_2 are the number of the runs in the respective samples compared. This gives the SD of the difference for run score averages.

SERIES: Several runs or experimental sessions that are grouped in accordance with a stated principle.

SESSION: A unit of an ESP or PK experiment comprising all the trials of one test occasion.

SET: A subdivision of the record page serving as a scoring unit for a consecutive group of trials, usually for the same target.

SEVENS TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling seven.

SIGNIFICANCE: A numerical result is significant when it equals or surpasses some criterion of degree of chance improbability. The criterion commonly used in this JOURNAL is a probability value of .02 or less, or a deviation in either direction such that the CR is 2.33 or greater.

SINGLES TEST: A PK technique in which the aim of the subject is to try to influence dice to fall with a specified face up.

STIMULUS OBJECT: The ESP card or drawing or other object, some identifying characteristic of which is to be apprehended by the percipient.

SUBJECT: The person who is experimented upon. In ESP tests, most commonly the percipient (though also the agent in GESP and telepathy). In PK tests, any individual whose task it is to influence the objects thrown.

TARGET: In ESP tests, the stimulus object; or in telepathy, the mental state of the agent. In PK tests, the faces of the die (or combination of faces) which the subject attempts to bring up in the act of throwing.

TARGET CARD: The card which the percipient is attempting to perceive (i.e., to identify or otherwise indicate a knowledge of).

TARGET DECK: The deck of cards the order of which the subject is attempting to identify.

TARGET FACE: The face on the die which the subject tries to turn up as a consequence of direct mental action.

TELEPATHY: Extrasensory perception of the mental activities of another person. It does not include the clairvoyant perception of objective events.

TRIAL: In ESP tests, a single attempt to identify a stimulus object. In PK tests, a single throw of the dice or other objects thrown.

A PSYCHOLOGIST LOOKS AT ESP

A final and extreme example of the present day: why do we not accept ESP as a psychological fact? Rhine has offered enough evidence to have convinced us on almost any other issue where one could make some guess as to the mechanics of the disputed process. Some of his evidence has been explained away, but as far as I can find out, not all of it. Until a complete rebuttal is provided or until we accept ESP, let us not talk about enlarging our notions of neurology to meet the psychological "facts" with no external criterion of what those facts are. We are still trying to find our way out of the magic wood of animism, where psychology began historically, and we cannot give up the talisman of a knowledge of material processes. Personally, I do not accept ESP for a moment, because it does not make sense. My external criteria, both of physics and of physiology, say that ESP is not a fact despite the behavioral evidence that has been reported. I cannot see what other basis my colleagues have for rejecting it; and if they are using my basis, they and I are allowing psychological evidence to be passed on by physical and physiological censors. Rhine may still turn out to be right, improbable as I think that is, and my own rejection of his views is—in the literal sense—prejudice.

—D. O. Hebb, "The Role of Neurological Ideas in Psychology," *The Journal of Personality*, September, 1951.